

PHILADELPHIA, APRIL 5, 1884.

ORIGINAL LECTURES.

CLINICAL LECTURE
ON THE PROGNOSIS IN CHRONIC
BRIGHT'S DISEASES.

Delivered in the Philadelphia Hospital

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THE question of prognosis in renal diseases, in common with similar questions in other forms of disease, is often vague and unsatisfactory. The three cases which I present to-day illustrate chronic forms of Bright's diseases, and I desire to use them to teach the paramount importance of the diagnosis of the cardiac and arterial changes in the prognosis of chronic Bright's diseases.

Physical examination reveals, in each instance, marked hypertrophy of the left ventricle of the heart; and the sphygmographic tracings show the oblique percussion-stroke, with its rounded apex, indicative of an increased arterial tension. The frequency of arterial and cardiac changes in the interstitial renal process is acknowledged on all sides. Dickinson places the proportion of cases in which the left ventricle is hypertrophied at thirty-one out of sixty-eight cases; Traube, at ninety-three out of one hundred; and Grainger Stewart says that, at a somewhat advanced stage, it is never entirely absent.

The parenchymatous forms of nephritis commonly pass through three stages: (1.) Stage of inflammation more or less pronounced. (2.) Stage of fatty transformation, which is more or less rapidly produced, since it may be developed in a few weeks or may continue for years. It is accompanied by a less diffuse and subacute form of inflammation than in the first stage. (3.) Stage of atrophy, presenting the smooth, granular, fatty kidney, more or less closely resembling the contracted interstitial kidney.

In certain cases of parenchymatous nephritis, hypertrophy of the heart also prevails, especially in those which have become chronic. An idea as to the relative frequency of hypertrophy of the heart in this condition of the renal substance can

be obtained from the writings of Grainger Stewart. He found hypertrophy of the heart present in only twelve per cent. of cases fatal in the first stage, thirty-eight per cent. of those fatal in the second stage, and one hundred per cent. of those fatal in the third stage. The hypertrophy of the heart is not nearly so frequent in the early stages. This may be explained by the great losses of albumen in cases at this time, by which their nutrition is profoundly affected and the nutrition of the heart equally so with the other organs.

Together with the hypertrophy of the heart there exist in chronic renal diseases certain arterial changes, representing arterial thickening due to an increase in the two outer coats of these vessels, which condition is often succeeded by atheroma. To make the proposition relating to the cardiac hypertrophy and vascular thickening apply to cases in general, we may say that both are due to capillary resistance and are consequences rather of chronic renal inadequacy, however produced, than belonging to any definite type of renal disease.

The frequency of cardiac and arterial lesions being conceded, we may appreciate their value in prognosis when we consider that the activity of other eliminative channels is dependent very largely upon the perfect performance of the circulation. The thorough elaboration of the food in the intestinal canal and liver is dependent upon the activity of the portal and mesenteric circulation. Indeed, the function of the liver is especially important in nutrition, since the discharge of imperfectly elaborated albuminoids through the hepatic vein is a potential cause of deterioration of the blood-tissue, and prepares the way for the development of uræmia. This condition is one in which the interchange of the nutritive elements of the blood with the waste elements of the tissues and their mutual replacement is seriously compromised. The nutrition of the cardiac muscle is among the first to be affected, and through cardiac failure the nutrition of the tissues in general becomes undermined. The importance of perfect cardiac nutrition in chronic renal disease is therefore manifest.

Probably the prognosis of chronic interstitial nephritis is especially involved in the question of the cardiac nutrition, since

all observers are agreed that failure of cardiac force may suddenly precipitate an attack of uræmia. But in chronic forms of nephritis with associated and parenchymatous changes, or in chronic parenchymatous nephritis, even waiving the question whether there is, in all chronic cases of long duration, a tendency to contracted kidney, the condition of the heart will prove the determining factor in diagnosis. In comparatively reviewing the general basis for prognosis in chronic forms of nephritis, one must consider the character of the other data upon which a prognosis may be based. The chemical quantitative examination of the urine for urea is important, but a compensative activity of the circulation will obviate the imperfect renal secretion, and the cardiac nutrition is sensitive to prolonged reduction in the activity of renal circulation.

The microscopical examination of the urine for casts as evidences of renal alterations is very significant, but the deductions from these are obviously most correct when considered together with the cardiac condition. In this way, in interstitial nephritis, the duration of the disease may be estimated, and mistakes due to inferences drawn from the presence of casts can be avoided, since recovery from renal disease has occurred even when casts indicative of the second stage of parenchymatous fatty degeneration have been found. The activity of the digestive system is an element in the prognosis, but this also depends on the efficiency of the mesenteric circulation. The stage and date of the disease also bear close relation to the prognosis, and should be studied in connection with the cardiac nutrition.

While the foregoing remarks must be regarded as general rules, remember that prolonged insufficient cutaneous activity, or acute exacerbations of renal mischief, or intercurrent processes in other organs, may at any moment place a period to the life of a patient. Therefore a prognosis on any form of chronic Bright's diseases should always include an "other things being equal."

The symptoms and complications calculated to alarm one in any form of Bright's diseases are suppression or diminution of the urine, nervous phenomena, unmanageable dropsy, chronic manifestations of uræmia, acute inflammation, and hemorrhages. Albuminuric retinitis is always a

serious symptom, occurring most frequently in cirrhotic forms of nephritis. In a recent paper read before the Philadelphia County Medical Society by Dr. W. S. Little, it was stated that among nine hundred and eleven cases of Bright's disease, reported by different observers, changes in the retina had been recorded in one hundred and eighty-five, or about twenty per cent. He continues, "Can a prognosis as to the duration of the disease, or as to its fatality, be derived from the eye-symptoms? Only in chronic states of the disease, where retinal hemorrhages are extensive and repeated, the heart being diseased. Brain-symptoms soon follow in these severe types of retinitis in chronic kidney-disease, a general hemorrhagic condition being developed, or uræmia may ensue. In the acute form of Bright's disease no prognosis of any value can be formed from the eye-symptoms, though severe."

I would add that an exception to the commonly serious character of albuminuric retinitis should be made in favor of pregnant women.

Finally, the progress of arterial lesions as associated with the cardiac lesions must be analyzed, especially to discover the presence of atheroma. These arterial lesions affect the cardiac nutrition by reducing aortic systole, and so interfering with the circulation through the coronary arteries. The progress of these changes can be successfully studied with the sphygmograph. Tracings should always be taken in chronic Bright's diseases, and the increase of arterial tension proportionate to the cardiac hypertrophy is a favorable sign, too high a grade unfavorable, indicating the surcharged state of the blood with unreduced albuminoids. The decrease below the standard is extremely unfavorable, since it is the precursor of degenerations and the harbinger of the various complications which terminate the clinical history of these cases. With the above facts in mind, our prognosis may stand as follows:

Parenchymatous nephritis is often quickly dangerous, but if a case survive the initial periods the prognosis in the early stages is less unfavorable to ultimate recovery than in the cirrhotic form. In the second stage, representing the stage of fatty transformation, recovery, though rare, is possible. The patient may survive

for a long period, although at any time death may ensue from sudden or gradual increase of the symptoms, or from intercurrent affections. In the third stage (*i.e.*, that of atrophy) the deterioration of general health renders the prognosis most unfavorable, yet even in this stage, if the cardiac nutrition be good and suitable hygienic and therapeutic conditions be fulfilled, life may still be prolonged for considerable periods. In the forms of renal disease cirrhotic from the outset, the progress of the disease is slow and insidious and the fatal result long deferred, yet the disease may be considerably advanced before it is detected, and the cardiac and arterial lesions materially assist us in approximating the stage of the process and its duration.

In conclusion, let me notice the sometimes favorable prognosis and short duration of attacks of subacute catarrhal nephritis occurring independently of scarlet fever in children. Recent publications in the English journals have called attention to such cases. They occur most often in children who suffer from enlarged tonsils and catarrhal tendencies. In such cases the ordinary symptoms of reduced urinary secretion, dropsy, etc., prevail; the urine is albuminous, and contains casts, without blood. All the symptoms may vanish in two to four weeks, and convalescence ensue, if appropriate treatment be instituted and favorable hygienic conditions exist.

ORIGINAL COMMUNICATIONS.

THE BACILLUS TUBERCULOSIS AND THE ETIOLOGY OF TUBERCULOSIS.—IS CONSUMPTION CONTAGIOUS?

SECOND COMMUNICATION.

Read before the Philadelphia County Medical Society,
November 14, 1883.

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VI.—EXPERIMENTS AND EVIDENCE, "PRO" AND "CONTRA."

IT has been shown that the clinical evidence in reference to the contagiousness of phthisis is so meagre that assertions as to its parasitic origin are unwarranted (see chap. iv.). Moreover, statistics negative

such theory. This being the case, it would seem as if experimenters are trying to prove that which is not the reality.

The testimony of the defenders of this theory, however, appears strengthened since the publication of the discovery of the bacillus and of the experiments of Koch. This is to be in a measure explained by the impression which Koch's well-constructed article made upon the minds of some of our leading clinical teachers, who involuntarily felt themselves induced to teach and to write about the doctrine of the contagiousness of phthisis. The profession at large does not care for Koch's discovery, whatever its value may be; but the opinion of the leading clinicians endorsing such discovery forms a guide, and may prove one of the most efficacious means of influencing the profession in regard to the question of the contagiousness of phthisis.

Having arrived, from my own experiments, at conclusions different from those of Koch, I thought it at present timely to announce at least the results of my observations, as my detailed report cannot appear for some months to come. It is my personal observations, together with my conclusions obtained from a careful perusal of the control experiments and of the records of the observations of others, which have determined my present attitude in the question of the etiology of tuberculosis.

The total evidence *pro* and *contra* gives me the impression that the doctrine of the contagious character and parasitic causation of tuberculosis cannot be sustained.

I will now submit a brief analysis and summary of experiments made and evidence offered in relation to the question of the parasitic origin and specific nature of tuberculosis.

For the establishment of a theory in regard to a parasitic origin of a disease by means of experiments on animals, etc., the following propositions must be affirmatively decided:

1. The disease produced experimentally in animals by means of inoculation with products of the human disease must be proved to be identical with the disease occurring spontaneously in man.

2. There should be some evidence showing that inoculation in man is followed by the same results as follow the inoculation of the same material in animals, and that the disease is really contagious.

3. There must be found a definite parasite at the beginning of the diseased process in all cases and in all tissues involved by the disease, and in sufficient quantity to account for the changes.

4. Given a parasite that is the cause of the disease, its action should be specific, —i.e., it alone should be the causative factor, and should, when isolated and inoculated into an animal liable to the disease, always produce that disease.

5. The lesions of a disease resulting from the inoculation of a specific parasite must also contain that parasite and the specific properties of reproducing the same disease when re-inoculated.

6. Finally, a given parasite and no other substance should, the conditions remaining the same, be capable of producing the disease.

Koch makes an effort to answer all the above propositions in the affirmative in reference to tuberculosis. As a thorough and experienced mycologist, he knew well that this is unavoidably necessary in order to establish the etiological relation of his bacillus to tuberculosis.

Tuberculosis was known before Koch to be inoculable, and was, upon popular notions and traditionally, regarded by some as a contagious disease. Taking such theory for granted, it was necessary to find the parasite. In fact, Klebs, Tous-saint, Max Schüller, and Aufrecht made excellent investigations, which even suggested the parasitic nature of tuberculosis, although the proofs offered by these investigators were not sufficient.

Koch's investigations, with his superior advantages, methods, and diligence, have been crowned with better success, and have brought forward facts of standing and permanent value to mycology, botany, and partly to medicine. His evidence in the question of the parasitic nature of tuberculosis is strong, but his conclusions from this evidence were overdrawn and too hasty. They are, thus far, not so much justified as he and his followers think they are. There is great lack of that absolute proof that is necessary for the settlement of a question of such magnitude and social importance.

Koch has, in relation to tuberculosis, brought forward definite affirmative proof for only some of the above-stated propositions, and this, again, *only partial*. Valuable contributions to this end have been

also made by others. But we must have *full proof for each and all* of those propositions, and these must be really applicable to tuberculosis, before we can accept the theory of a parasitic character and of the contagiousness of this disease.

Submitting, now, a brief criticism of the bacillus theory of Koch and his followers, I will take up separately each of the above-stated propositions, all of which it is necessary to prove in the affirmative before there is any reason for the establishment of such a theory in regard to tuberculosis.

1. *The disease produced experimentally in animals by means of inoculation with products of human disease must be proved to be identical with the disease occurring spontaneously in man.*

In favor of the identity of human tuberculosis with that produced experimentally in animals there has been brought forward the fact that the products in both contain identical bacilli. But this surely does not prove the identity, because similar bacilli may be found in the lesions of various kinds of processes, resulting in cheesy products. (See bacillus chapter.) Besides, there are many spontaneous and artificially-induced tubercular lesions in which bacilli could not be demonstrated. Hence we cannot rely upon the bacilli as a proof for the identity of the lesions.

Koch and those who imitated his experiments diagnosticate and declare all those artificially-induced lesions as tubercular which occur in nodes and in which they found the tubercle-bacillus, without taking (so far as I know) into consideration any structural peculiarities or other conditions. Now, tubercle-bacilli will surely be found in the lesions, whatever these may be, as they were introduced into the animal in those experiments. Further, in the opinion of these gentlemen nothing is tubercle where there are no tubercle-bacilli. Therefore, how can we rely upon their statements as to what the lesions they induced in animals really were?

It is hardly within the province of the mycologist to teach us what is tubercle and what is not tubercle.

Tuberculous lesions with extensive cheesy changes and tissue-destruction, cavities, etc., such as occur spontaneously and often quite speedily in man or animals, cannot be induced experimentally by means of inoculation, unless very large

quantities of some purulent tuberculous materials are used, and abscesses result. When an animal dies several or many months after the operation of natural tuberculosis, extensive caseation of the organs may occur.* The only kind of induced or artificial tuberculosis in animals which may be ascribed to the effects of inoculation is one that corresponds in naked-eye appearance to secondary miliary eruption of tubercle as occurring in man,—the acute miliary tuberculosis. This acute miliary tuberculosis in man, which I observed also in animals as a spontaneous disease, occurs only in wasting diseases accompanied by various grave symptoms, anæmia, and great emaciation; while the induced disease in animals occurs suddenly, and induces no symptoms, no blood-changes, no emaciation, etc.

In many instances where the experimenters have produced, by means of tuberculous materials, within two to eight days after the operation, a miliary eruption, it is not probable that those miliary nodes were tubercles, and were due to the effects of bacilli, which are known to grow extremely slowly, and it is not certain that the experimenters took pains to distinguish them from true tubercle, or were competent in all instances to do so. This is eminently true of the inhalation tuberculosis.

Tappeiner's induced inhalation tuberculosis of dogs,† so much relied upon by Koch and others for the establishment of the mode of the spreading of phthisis, and partly of the bacillus doctrine itself, has been proved to be a fiction. Tappeiner, as so often quoted, subjected dogs to an atmosphere heavily charged with phthisical sputum, so that the dogs were nearly bathed in the latter (known to contain bacilli) for weeks. But, in spite of this, the animals grew fat, if anything, and, after the lapse of a certain time, acquired local pulmonary affections in the form of nodules, not likely to have been tubercular in nature, of which only in one case were some observed in the liver and kidneys.

The experiments of Schottelius,‡ War-

gunin and Rajewsky,§ Weichselbaum,|| and of others,¶ and my own experiments also (to be reported subsequently), make Tappeiner's assertions perfectly untenable. Tappeiner's own account of his experiments and the microscopical description of the structure of Tappeiner's "tubercles" by Grawitz and Friedlander in Virchow's institute clearly indicate that he had nodular broncho-pneumonic foci, and not tubercles. (See explanation of these formations in the first chapter of this paper.)

I will, however, show later that pulmonary tuberculosis may occasionally be produced in rabbits by these means.

Furthermore, the bacillus theorists assert that inoculation-experiments, and these alone, can prove the tubercular nature of the nodular eruptions obtained artificially in animals. The identity with human tubercle is considered established, because inoculation nodes from the animal and tubercles of man act alike. They claim that whatever can produce tubercle on inoculation contains the tubercular virus, and is tubercle. Under such conception, finely-powdered sterilized glass should be classed with tubercle, because it, as I can reaffirm now, is capable of inducing tuberculosis when introduced into the tissues of healthy animals.

The following deserves a passing mention. According to Orth** and Bollinger,†† there is some doubt as to the identity of human and animal tuberculosis. The results of the experiments of both these observers show that tuberculosis could only be induced by feeding animals with materials from animal tuberculosis; while tuberculous materials taken from man had no effect upon animals when given as food. On the other hand, the Würzburg feeding experiments upon man‡‡ prove that animal tuberculous materials have no effect on man.

Although, judging from my own experiments, there is to my mind no doubt that some forms of artificially-induced tuberculosis in animals acquire gradually characters which make them identical with the

* I was much surprised last summer to see in Berlin, at the Hygienic Exhibition, in Koch's pavilion, specimens of the character just stated exhibited as inoculation-tuberculosis, and still more to hear the demonstrator explain (surely without being authorized by Koch) that these specimens were to demonstrate the rapid effects of the bacillus.

† Virchow's Arch., lxxiv., 1878, and *ibid.*, lxxvii., 1880.

‡ Virchow's Arch., lxxii., 1876, and *ibid.*, xci., 1883.

§ Vratsch, No. 6, 1882.

|| Centralblatt, No. 19, 1882.

¶ To the same conclusion, I hope, will also come my esteemed friend Prof. Brose, if he repeats his experiments published in the Medical Record, January, 1884.

** Virchow's Archives, vol. lxxvi.

†† Arch. f. Exper. Path., vol. i.

‡‡ Schottelius, *loc. cit.*

spontaneous tuberculosis in man or beast, yet I do not think it is at all proved that the lesions so rapidly arising from the effects of the inoculation with the bacillus of Koch are identical with tuberculosis in man. The proof, then, upon this point, the supreme one for the settlement of the question of the nature of tuberculosis, is yet to be furnished.

2. *There should be some evidence showing that inoculation in men is followed by the same result as follows the inoculation of the same material in animals, and that the disease is really contagious.*

In favor of the direct inoculability of tuberculosis in man the following is presented:

At a recent meeting of the Académie de Médecine, M. Verneuil related the following history. In July, 1877, a house surgeon (*interne*) at the Ste. Eugénie Hospital, who performed all the post-mortem examinations, one day noticed a papule at the base of the nail of his third finger. The apex presented a white spot, and a few drops of pus escaped from it. It was frequently cauterized, but the phalanges became attacked, and a cold abscess spread over the back of the hand. After three years' treatment, having failed to produce any improvement, M. Verneuil amputated the finger. The house surgeon was believed to be cured, and practised in the provinces. Quite recently he has been again attacked by cold abscesses in the lumbar region, causing intense pain; during violent attacks of pain the arms exhibit clonic convulsive movements. M. Verneuil has operated a second time. He is convinced that his patient became inoculated with tuberculosis when performing a necropsy. A similar misfortune happened to Laennec. One day, when operating on a tuberculous patient, he slightly cut himself with a saw. A swelling appeared on the wounded part. Laennec cauterized it with antimony chloride. The swelling disappeared, but twenty years subsequently he died from tuberculosis.*

We have seen that clinical evidence and statistics do not elucidate a contagion for tuberculosis, and that the few isolated instances of apparent contagion offered cannot stand the test of scientific scrutiny. An infectious or contagious disease can have only one cause, and cannot be at

one time due to a contagion and at other times arise from a variety of causes: hence the latter part of the proposition must be answered in the negative.

This being the case, the *parasitic* origin must also be denied it, as a necessary consequence.

As to the first part of the proposition, too little is known of scientific observation upon this point in regard to tuberculosis. According to the exhaustive investigation of Dr. Law,† there is no evidence that tuberculosis has ever been conveyed through vaccination.

I must mention, though, an actual inoculation-experiment upon man, not so much on account of its inherent value, as because it has been quoted with great reliance in support of the infectiousness of tuberculosis. Demet, Paraskeve, and Zalonis, in Syra, Greece,‡ “inoculated a man of 55 with tubercle. He was suffering from gangrene of the left great toe, due to the obliteration of the femoral artery, and his death was inevitable, as he had refused to submit to amputation. His lungs were carefully examined and found to be sound. They inoculated the upper portion of the right leg with sputa from a man who had abscesses in his lung. Three weeks later there were signs of commencing induration at the summit of the right lung. The patient died on the thirty-eighth day after the inoculation, from gangrene. At the necropsy there were found at the apex of the right lung seventeen small tubercles, varying in size from that of a mustard-seed to that of a lentil. Two similar tubercles were found in the left apex, and two others in the liver. The experimenters concluded that the embryonic state of the tubercles and their limited number were due to the short time since the inoculation.”

This isolated experiment, as well as any of the experiments on animals, is valid only when we take it for granted that the experimenters are able to differentiate spontaneous from artificially-induced tuberculosis. This is not probable in the case just quoted. We are told that the man experimented upon was suffering from an exhausting disease, and it is well known that at least one-third of the autopsies in such cases reveal tubercular disease.

* National Board of Health Bulletin, No. 40, 1882.

† Quoted by Med.-Chir. Review, October, 1874, from Gazette Médicale, 1872, page 192.

* Paris Correspondence of the British Medical Journal, quoted by Boston Med. and Surg. Journal, No. 10.

Directly bearing upon the proposition under consideration are again those Würzburg feeding-experiments, in which material known to be infested by tubercle-bacilli was used, often raw, for years as food, under strictly scientific supervision, with absolutely negative results, and which tended to show that man does not react at all upon the tubercle-bacillus.

3. *There should be found a definite parasite at the beginning of the diseased process, and in sufficient quantity to account for the changes in all cases and in all tissues involved by the disease.*

In relation to tuberculosis this proposition cannot be answered in the affirmative; and it is by no means as definitely settled as some high clinical authorities hold with Koch, that there is but one "specific parasite" in tuberculosis.

Klebs,* Toussaint, and Schüller† have observed *micrococci* to be constantly present in tuberculous lesions and products (and have induced artificially the disease with the isolated micrococci), and no one has *proved* anything to the contrary; while Koch and Baumgarten‡ discovered *bacilli* in the same lesions. Koch claims for his bacillus more than is consistent with the laws of physiological and pathological life and sound argumentation, and more than is in correspondence with the actual proofs offered in relation to the pathogenetic properties of this bacillus.

The reports of some competent microscopists and pathologists (when the originals are examined) show that the tubercle-bacillus is not invariably present in all cases and all products of tuberculosis; and, if present, it is often not seen in sufficient quantity to ascribe to it the claimed significance; and, furthermore, it is, as a rule, not present in the beginning of the disease. On grounds of personal investigation I can offer similar testimony.

The bacilli should be present in every lesion and in all cases and in the beginning of tubercular disease, and not chiefly in its degenerated products, if tuberculosis is to be called a parasitic disease, in accordance with the laws of pathology. In all well-established parasitic diseases the parasite is a necessary factor and is invariably

present,—unless there should be established for the "tubercle parasite" an exceptional, new, and mysterious mode of action.

The truth of the matter appears to be, and, indeed, from my daily observations in the laboratory upon a large quantity of material, I regard it as a fact, that the tubercle-bacillus of Koch is a mere concomitant of cheesy disintegrated materials, even if it be pre-eminently of tuberculous cheesy materials.

4. *Given a parasite that is the cause of a disease, it should, when isolated and inoculated into an animal liable to that disease, always reproduce that disease; but its action should be specific,—i.e., it (the parasite) alone should be the causative factor.*

There is no doubt that Koch's tubercle-bacillus when isolated and cultivated for many generations and then inoculated into certain animals is capable of inducing tuberculosis, or a nodular eruption not distinguishable from it, more readily than other irritants, so far as tried. Success in inoculating is particularly frequent in rabbits and guinea-pigs (although not so common as Koch claims), but only conditional and rare in other animals.

Thus it appears that the above proposition could be answered for tuberculosis and the bacillus in the affirmative if only the following points were proved:

1st. That the nodular lesion thus induced is really tuberculosis, identical with the human disease.

2d. That this bacillus is the only bacterium or the only irritant capable of inducing tuberculosis; and,

3d. That its action is specific,—i.e., that the bacillus is the only agency or factor at work, the sole cause of the disease.

The first point is not proved, as probable as it may appear. The other points are open to the following considerations and objections:

It has been proved that in tuberculosis micrococci, as well as bacilli, are causal, the evidence being "strong" for either "parasite;" whereas the bacillus alone should be the causal factor. As long as not *disproved*, Klebs's, Toussaint's, and Schüller's investigations (in relation to the micrococci as causal factors) have as much claim as Koch's. The method of cultivating those tubercle-micrococci, as practised by those investigators, was one not favorable for the development of the tubercle-bacilli. Further, Watson Cheyne's

* Klebs now admits the bacilli, but denies that they are invariably present, and denies on grounds of experiments their exclusive pathogenetic properties, although he admits that they are a not unimportant admixture to his micrococci.

† Loc. cit.

‡ Loc. cit.

assertion that bacilli *must have been* present in the cultured materials with which those investigators inoculated successfully, is altogether a gratuitous assumption, and his few and imperfect control experiments with Toussaint's micrococci were not satisfactory, and, in fact, neither prove nor disprove anything.* Koch did not try the effects of any other fungus than that of his bacillus in relation to tuberculosis.

Koch further claims that the specific character of his bacillus is supported by the rapidity of its effects, and brings forward the inhalation experiments of Tappeiner and the experiments upon the eye. The former I have shown to be valueless, as those nodules produced in the lung, especially if rapidly formed, are not tubercles. I have also reason to believe that the same is the case with many of the experiments on the eye, especially in those cases in which an apparently acute miliary tuberculosis of the lung rapidly followed the inoculation: in fact, in some instances this eruption occurred in a much shorter time than is at all possible for tubercles to develop.

Koch has not proved that his bacillus is the only agency at work in the production of tuberculosis. Although he undoubtedly inoculated the pure bacillus, he ignored the specific reaction of the soil; and it is the latter which I hold plays the most important rôle in determining the formation of tubercle. In introducing the bacillus into the animal organism, another factor—the injury inflicted, and its effects upon the living cells of the body—must be taken into consideration.

In some animals all the tissues of the body react equally upon the introduction of irritants; in others only some one of the tissues responds, such as the serous membranes. This surely demonstrates the specific action of the soil.

I must again call attention to the fact that in making his experiments Koch injected the bacilli into any part indiscriminately in scrofulous animals, while in non-scrofulous animals (dogs, rats, cats) he injected them only into the peritoneum or the anterior chamber of the eye, where, we know from experience and from repeated experiments, any irritant of sufficient intensity may create tuberculosis.

This cannot be explained by the assump-

tion of Koch that the bacillus must merely be enclosed in something so as not to be eliminated before it can exert its effect.

To me it appears that the reason why we must inoculate in serous cavities to produce tuberculosis in the dog or cat, is, because we want not so much the specific action of the irritant (say of the bacilli) as the properties of the serous membranes. It is now well known that any chronic inflammation of serous membranes may lead to primary tuberculosis. It is proved that we do have a primary tubercular synovitis or a primary tuberculous pericarditis; and that bacilli could be instrumental in its production is highly improbable.

In surface tuberculosis like that of the lung the bacilli, in my opinion, also play only a secondary rôle.

Koch himself admits that it is not likely that the bacillus when inhaled by man could get a foothold in a normal lung. He says distinctly in his original articles that the lung must be predisposed for the reception and the action of the bacilli. Under such predisposition he understands and enumerates the following lesions: "*desquamation of the epithelial lining of the respiratory tract, stagnating exudates and secretions in the lung, adhesions, anomalies of respiration,*" etc. Now, here is a matter of mere interpretation of these lesions. Koch innocently calls them "*predispositions,*" while every pathologist will designate some of these lesions as suggesting already-existing pulmonary phthisis. In fact, at the present standing of our knowledge of pulmonary phthisis we can have no desquamation of the vesicular epithelium without preceding tubercular infiltration.

Watson Cheyne is also considerate enough to say,† "*... it seems to me that the lung must in addition be prepared for the reception of the bacillus, as may be the case if congestion or slight inflammation be present at the time of the inhalation of the organism.*"

That in inoculations into serous cavities the latter do not act merely in preventing the bacillus from escaping or being eliminated, and that the stagnating secretions in the lung do not act merely as a glue to retain the bacillus in order to allow the accomplishment of its effects, is, to my mind, proved by the following experiments of

* See Watson Cheyne's report, *Practitioner*, April, 1883, pp. 272-276.

† Loc. cit., p. 314.

Bollinger. Bollinger,* in order to show that tuberculosis could not be transplanted by vaccination, made superficial *cutaneous* inoculation in rabbits with tuberculous materials with negative, and deep *subcutaneous* inoculations with the same material, followed by intense inflammation, with positive results. In both cases the wounds were covered by a layer of collodion to prevent the "elimination" of the bacillus.

Thus it appears that the bacilli by themselves have no effect upon the healthy organism or the normal tissues. A predisposed soil is the chief factor and is pre-eminently necessary for the production of tuberculosis; while, on the other hand, it is not proved at all that the bacillus is invariably necessary for the production of tuberculous lesions. Although the tubercle-bacillus is more liable to excite tuberculosis in an already inflamed and ill-nourished soil than all other simple irritants so far tested, it (the bacillus) might be readily substituted by other irritants.

The matter must unquestionably be tested further, but from the above evidence it is clear that a general fear of the bacillus tuberculosis as a contagion is unjustifiable, and that the ordinary dust suspended in the air is to certain persons as dangerous as the bacillus.

5. *The specific lesions of a disease resulting from the inoculation of a specific parasite must also contain that parasite, and have the specific properties of reproducing the same disease when re-inoculated in other animals.*

Koch claims that the products obtained in animals by inoculation with bacilli are capable of producing tuberculosis when inoculated into a second animal, while the products obtained by inoculation with innocuous substances do not have this effect. The former proposition is true, but the latter, I hold, is not in accordance with facts. In my own experiments, to be detailed in my forthcoming report, tubercles produced by inoculation with innocuous material under antiseptic precautions were likewise capable of producing tubercles when inoculated into other animals, having thus the same action as the innocuous material primarily used.

I have also shown above (see bacillus chapter) that in secondary tuberculous products bacilli may be absent.

The experiments of Martin,† which tend to show even the progressive virulence of products obtained from re-inoculation with tuberculous material in a series of animals, have been substantiated by no one.

Martin's assertion also, that inoculations with products obtained by the introduction of innocuous substances never produce true tuberculosis, and that after a series of re-inoculations these products lose their power of acting even as local irritants, is, according to control experiments, positively wrong. On the other hand, views have been expressed, based upon experiments (I think also by Martin), that products obtained by inoculation with non-tuberculous substances when re-inoculated may gradually become specific, and increase in virulence in producing tuberculosis.

6. *Finally, a given parasite and no other substance should, the conditions remaining the same, be capable of producing a parasitic disease.*

In my previous studies, judging from the literature alone, I was fully impressed with the idea that tuberculosis had a specific exciting cause, and that it could be induced by inoculation with tuberculous materials. Moreover, having made numerous inoculations with tuberculous matters, I convinced myself of this fact. Hence I accepted the view that tuberculosis is inoculable in certain animals.

But, at the same time, after repeating, under various modifications, the well-known control experiments, I found that, beyond doubt, even true tuberculosis could be induced by substances other than tubercular, and that failures to induce tuberculosis with tuberculous materials were in certain animals nearly as common as successful inoculations with innocuous substances.

To these experiments I will return in my forthcoming report.

It will be also necessary to first consider the evidence of those observers who, from the results of their own exhaustive experiments, negated the exclusive or specific infectious properties of tuberculous materials. This negative evidence is by far more voluminous and strong than the admirers of the hypothesis of the contagiousness of tuberculosis suppose; excited admirers having especially arisen

* Zur Aetiologie der Tuberculose, Prager Med. Wochenschrift, Nos. 4 and 5, 1884.

† Journal d'Anatomie et de Physiologie, April, 1881.

since the ingenious article of Koch appeared.

It is, however, remarkable that some of the writers on tuberculosis fail to understand that the pivot of the question of the etiology of tuberculosis does not rest upon the fact alone whether or not the bacillus induces lesions analogous to tuberculosis, but pre-eminently upon the fact whether innocuous substances have or have not the same effects.

Thus, above all, the negative evidence must be carefully inquired into, not by relying upon the crippled and sometimes misrepresenting and meagre quotations of some of the compiling writers, but by submitting the original communications of the authors and experimenters to a careful perusal.

Together with the accounts of the much-quoted experiments of investigators who succeeded in inducing tuberculosis in animals with tuberculous substances only, the reading and thorough examination of the records and thorough examination of the observers to be mentioned below are unavoidably necessary.

The following observers all refer to many or few experiments of their own in which tuberculosis resulted from the inoculation with either innocuous substances or with specific matters other than tuberculous:

- Lebert, *Allgem. Med. Central-Zeitung*, 1866.
 Lebert and Wyss, *Virchow's Archiv*, vol. xl., 1867.
 Empis, *Report of the Paris Internat. Med. Congress*, 1867.
 Burdon Sanderson, *British Med. Journal*, 1868.
 Wilson Fox, *British Med. Journal*, 1868.
 Langhans, *Habilitationschrift*, Marburg, 1867.
 Clark, *The Medical Times*, 1867.
 Waldenburg, *Die Tuberculose*, etc., Berlin, 1869.
 Papillon, Nicol, and Leveran, *Gaz. des Hôp.*, 1871.
 Bernhardt, *Deutsch. Arch. f. Klin. Med.*, 1869.
 Gerlach, *Virchow's Archiv*, vol. li., 1870.
 Foulis, *Glasgow Med. Journal*, 1875.
 Perls, *Allgemeine Pathologie*, 1877.
 Grohe, *Berliner Klin. Wochenschr.*, No. 1, 1870.
 Cohnheim and Fränkel, *Virchow's Archiv*, vol. xlv., 1869.
 Knauff, *4te Versamml. Deutsch. Naturforscher*, Frankfurt.
 Ins, *Arch. f. Experim. Pathologie*, vol. v., 1876.
 Wolff, *Virchow's Archiv*, vol. lxxvii., 1867.
 Ruppert, *Virchow's Archiv*, vol. lxxii., 1878.
 Schottelius, *Virchow's Archiv*, vol. lxxiii., 1878; *ibid.*, xli., 1883.
 Virchow, *Virchow's Archiv*, vol. lxxvii., 1880.
 Stricker, *Vorlesungen über Exp. Pathologie*, Wien, 1879.
 Martin, *Med. Centralblatt*, 1880. No. 42.
 Wood and Formad, *National Board of Health Bulletin*, Supplement No. 7, 1880.
 Robinson, *Philadelphia Med. Times*, 1881.
 Weichselbaum, *Med. Centralblatt*, No. 19, 1882, and *Med. Jahrbücher*, 1883.
 Balough, *Wiener Mediz. Blätter*, No. 49, 1882.
 Warguin, *Allg. Med. Centralblatt*, April 8, 1882.
 Hänsell, *Arch. f. Ophthalmologie*, vol. xxv.

* Hänsell, who inoculated animals with gummous growths and syphilitic pus, obtained an exquisite miliary tuberculosis from the effect of these substances. In this connection may also be mentioned the following:

Damsch (*Centralbl. f. Med. Wissen.*, July 21, 1883), who obtained tubercular eruptions and nodes in the brain in rabbits

Some of the observers enumerated did not consider the miliary eruptions obtained experimentally as true tubercles, but the majority did so, and, as I will show later, presented excellent and reliable experiments and sound reasoning in support of their views.

Shall all the above evidence go for naught merely because Koch has discovered a bacillus which is capable of inducing in animals lesions resembling tuberculosis?

I trust it will not. Koch has, so far, no authority to claim *exclusive* pathogenetic properties for his bacillus, as he made himself no satisfactory control experiments with substances other than tuberculous. The few control experiments he offers, viz., that *sterilized* blood-serum (!), tuberculous material soaked in alcohol, and fresh scrofulous glands, or pus from tuberculous lesions, did not induce tuberculosis, prove little or nothing in favor of his bacillus.

Watson Cheyne, in his excellent report, displayed great care, diligence, and skill in his experiments and observations intended to corroborate Koch, but in making his control experiments he likewise was not very particular. So in relation to inoculations with non-tuberculous substances he came to the conclusion that "not one of the twenty animals (inoculated with innocuous substances) became tuberculous"! But when the detailed account of Watson Cheyne's experiments is read over, it is amusing to learn that only nine out of the twenty-five supposed negative experiments were really known to be negative, because eleven of the rabbits experimented upon had been stolen before Cheyne had a chance to examine them, two rabbits died within a few days, or long before tubercle could develop, and in three rabbits the experimenter really records lesions that might have been tuberculous, in spite of the absence of ba-

through inoculation into the eye with the cultivated bacilli of leprosy. Similar inoculation with leprosy material led to a perfect miliary tuberculosis in rabbits in the hands of Kaposi, of Vienna (*Wiener Med. Presse*, January 21, 1883).

Pfeiffer, Dotselpont, Cornil and Babès (loc. cit.) had the same experience with lupus material.

Bodamer (Inaugural Thesis, Univ. of Penn., 1884) had, as the result of inoculating with the pure cultivated actinomycetes fungus, a striking general miliary tuberculosis in rabbits.

Inoculation with materials from glanders gives also rise to tubercles in the lungs, etc., not distinguishable under the microscope from true miliary tuberculosis. But Löffler, who kindly demonstrated to me this fact in Koch's laboratory, and who also gave me a specimen demonstrating it, explained that the nodules in the lungs were not tubercle, because the bacilli found therein behave differently in staining.

† Loc. cit.

cilli in them, which latter circumstance, however, induced him to call the result a negative one.

These are instances of the way in which experimenters with preconceived and peculiar ideas upon a subject may unconsciously be misled in forming conclusions from their own experiments.

Further, it is interesting to note that in the "classical" experiments of Solomonson,* Baumgarten,† Tapeiner,‡ etc., among other substances, the following materials were used extensively for control: "caseous glands from scrofulous child," "caseous material from various sources," "muscle, testicle, and kidney from tuberculous guinea-pig," "cheesy pus from man and animals, cheesy infarcts, caseous tumors," etc. All these substances, which are known usually to contain the bacillus, were inoculated while fresh into animals, and are recorded by the experimenters above stated as having failed to produce tuberculosis. This is surely not consistent with the doctrine of Koch.

Wherever inoculation with innocuous substances was followed by positive results, the over-zealous germ-theorists call it "*accidental tuberculosis*." They say that at the time of former experiments the communicability of tubercle by a mediate contagion was not recognized, and as the precautions necessary for thorough disinfection of instruments, surroundings, etc., were probably not observed, the channels for the introduction of the *bacillus* were in all previous experiments left unguarded: hence, they argue, *it must have been* this ubiquitous bacillus which induced the tubercle.§

* Aftryk fra Nord. Med. Arkiv, vol. xi., 1879.

† Loc. cit.

‡ Loc. cit.

§ In this connection the following incident is interesting, particularly on account of the high authority of the observer:

Some experiments were made under the supervision of Virchow (*Berlin. Klin. Woch.*, 1880), principally with the view of testing whether the milk of animals affected with "pearl-disease," or bovine tuberculosis could reproduce the disease when fed to other animals. Virchow's own objection to experiments of this kind is that the various chronic inflammatory processes which occur spontaneously in animals are not sufficiently well known even to veterinary specialists. Further, in pigs, which he used in considerable numbers, scrofulous glands occur so frequently, from their alliance to man through their omnivorous habits, and their detection during life is a matter of such great difficulty, that results founded upon their presence must be accepted with great caution. The possibility of coincidence was also well illustrated by two cases in which several animals were found to be tuberculous after having taken the milk for some time from a cow which was diagnosed during life as affected with bovine tuberculosis, but whose lungs were found at the autopsy filled with echinococcus cysts, and with no trace of tuberculosis.

The milk of another animal which subsequently was found to be profusely affected by bovine tuberculosis had, on the other hand, no effect when given as food to a number of healthy animals.

The only result that Virchow thinks is perhaps justified from these experiments, is that more animals were found to be

Further admitting, however, that innocuous substances may induce tubercle-like bodies, they claim that these bodies are not infectious, *i.e.*, they are *false tubercles*.

All these objections would be very plausible if they were based upon actual observations and facts; but, unfortunately for the bacillus theory, they are not: they are *mere unfounded assumptions*.

The fact established by experiments, that a true tuberculosis can be induced in animals by inoculation with innocuous and various other substances, and the significance of this fact, can surely not be overthrown by the imperfect evidence that the bacillus is more liable to do so, and still less by the mere unauthorized opinions of some of the writers.

Erroneous conclusions and views may easily be formed through the misconception of the significance of experiments.

At the meeting of the Pathological Society of London (December 4, 1883, quoted from the *Lancet*, December 8, 1883), Dr. Wilson Fox announced the following: "He was unwilling that his former observations|| should still be quoted as opposed to the doctrines of Koch and those who had been more recently working at the subject; and therefore he had felt bound to come forward and make known the modification which his views had undergone. At the same time," Dr. Fox, however, added, "there was perhaps some danger of phthisiophobia or phthisiomania. During the past thirty years there had been many changes in the doctrine of phthisis, and hardly any doctrine has lasted more than five years."

But what had happened to induce Dr. Fox to lose faith in his own honest and excellent former work? So far as I could learn, it was the following. Dr. Fox had requested a Dr. Dawson Williams to repeat his former experiments. This bacillus-excited gentleman introduced *carefully* some "*putrid fluids*" and some *setons* into a few guinea-pigs and—did not obtain tuberculosis! Now, they think, it was at once evident that in all the former successful inoculations with non-tuberculous materials the mischievous bacillus of Koch must have gained entrance.

tuberculous among a certain number which had been fed upon the "pearly" milk than among the same number which had been fed upon healthy milk.

(The above statements, first quoted by Dr. Whitney, of Boston, Professor Virchow corroborated in a conversation with me upon this subject last summer. H. F.)

|| Loc. cit.

The reasoning of the London gentlemen appears to have been here as follows: *Putrid matter and setons do not induce tuberculosis; but the bacillus does. Hence the bacillus is the sole specific cause!*

But what is gained or proved for the bacillus theory if any one given substance, when inoculated into an animal, does not induce tuberculosis? Does, through this, the necessity of contagion at once arise? Surely not. If, for instance, as I will prove, finely-powdered, sterilized glass is capable of inducing a true tuberculosis, then it does not matter if putrid matter or setons failed to do it.

Cohnheim's acceptance of a theory of a specific poison for tuberculosis, which formed as its direct outgrowth, the basis of the bacillus theory, was also not justified from Cohnheim's own experiments. If he once succeeded* with innocuous substances in producing *peritoneal* tuberculosis, it is of no consequence that he subsequently† failed to induce an *iris tuberculosis*.

Negative results prove nothing under the above circumstances and in the presence of positive results. Most of the observations made in bacillus studies prove really nothing for the etiology of tuberculosis, and some interpretations of the results of experiments in this direction are quite deficient and not consistent with the principles of experimental pathology. Furthermore, some of the positive evidence must be excluded on account of the evident deficient knowledge of pathological anatomy on the part of some of the experimenters.

I am glad to be in the position to offer in my next communication a new series of observations and experiments on tuberculosis. These experiments, instituted under the auspices of Dr. Pepper, provost of the University of Pennsylvania, and executed by myself and assistants under all rules of scientific precautions and with full facilities for such work, plainly demonstrate that *the etiology of tuberculosis does not rest with Koch's "parasitic" bacillus or any other "contagion."*

The experiments referred to will be given in full details in a special report now in progress and soon to be published with appropriate illustrations, etc.

I desire, however, to announce here

that *my experiments prove that finely-powdered, sterilized glass, ultramarine blue, and other substances are by themselves capable of producing tuberculosis in animals or tissues liable to this affection.*

Further, I will offer proof that this effect (tuberculosis) ensues without the intercurrent action of any bacterium. And, finally, that in those instances where miliary, nodular eruptions have been induced by the tubercle-bacillus (or substances containing it), the action of the latter is a purely mechanical one, like that of simple irritants.

Further, these experiments show that the only advantage which the bacilli have over other finely-divided matter and simple irritants is that the former multiply and thus intensify their action, while mechanical irritants have not this property, and hence must be introduced in larger quantities. The more finely divided the matter, the more prompt seems to be its effect, and I believe it is impossible to render any matter more finely divided than the bacilli.

Like others, I also often succeeded in tracing the formation of the tubercle-nodules to the effects of the irritating particulate matter, if the latter were or could be made distinct enough to be seen within the nodes. When ultramarine blue was used for inoculation, granules of the latter substance were seen within the nodes; when bacilli were used to that end, then bacilli could be detected within the nodes. But in either case these primary nodular eruptions, *if rapidly formed*, do not yet represent tuberculosis, as I will show.

It is generally conceived that a specific infectious disease, such as instanced by variola, syphilis, anthrax, etc., can have only one cause or one poison which will produce that disease and nothing else, and cannot be substituted by anything else.

For tuberculosis this is not true, for we have bacillary and non-bacillary forms of tuberculosis.

It is now no more a question of observation and experimentation, but rather one of interpretation and understanding of the results; for we have seen that the evidence from experiments and microscopical studies is nearly sufficient.

But there are misconceptions. If that only is tuberculosis where the bacillus of Koch is found, or that only which arises from the effects of this bacillus, then Koch's theory of the exclusive pathoge-

* Loc. cit.

† Sitzungsberichte d. Schlesischen Gesell., 1878.

tic properties of the bacillus is correct, and under such a definition tuberculosis has only one cause. But if true tubercles exist and can be produced without the bacillus, which has been shown to be the fact, then Koch's theory cannot be accepted from a pathologico-anatomical stand-point; or else we are obliged to admit two or more kinds of tuberculosis,—one due to Koch's parasite, and others to a variety of causes.*

So far, however, we have no reason, from a pathologico-anatomical stand-point, to subdivide tuberculosis, and therefore I am of the opinion that Koch's view of the exclusive pathogenetic property of his tubercle-bacillus is decidedly overdrawn and even not warranted by facts. Neither the specific action of Koch's bacillus, nor the specific character of tubercle, nor the contagiousness of phthisis or of any form of tuberculosis, is proved.

Only after a complete harmony of the facts derived from pathologico-anatomical, experimental, and clinical studies in tuberculosis with those revealed by mycology, and not from either of these alone, can we arrive at a settlement of the question of the etiology of tuberculosis.

Further details concerning this question will be incorporated in my report. This will embrace also studies into the onset and the distribution of tuberculous affections.

From the above analysis of the bacillus question and of the etiology of tuberculosis the conclusions follow—

1. That the bacillus of Koch is a valuable diagnostic sign of tubercular disease.
2. That nothing is proved by its discovery for the etiology of tuberculosis.
3. That the too ready acceptance of the bacillus doctrine is not justifiable, and is likely to do more harm than good.
4. That neither phthisis nor any form of tuberculosis is contagious.

THE PHILADELPHIA SCHOOL OF ANATOMY is now open for the spring course of lectures under Dr. George McClellan, who will lecture upon the anatomy of the eye, ear, and internal organs.

* A suggestion to separate an "infective" form of phthisis from ordinary phthisis has been made by Dr. Reginald Thompson (*London Lancet*, No. 6, 1880, quoted by R. S. Smith, Bristol, *Medico-Chir. Journal*, No. 1, 1883). "In a series of fifteen thousand cases observed, fifteen cases (only one per one thousand) proved to be of an infective kind,—viz., with history of contagion and absence of phthisical family history."

REPORT ON THE PROGRESS OF OPHTHALMOLOGY.

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BRAIN AND EYE.

DR. NIEDEN, of Bochum, contributes some clinical records to the well-recognized but poorly-understood subject of the relations between intracranial disease and the eye. The following abstracts are necessarily brief, but contain the leading points.

Case I.—Mr. M., æt. 25, six months ago received a violent blow on the back of the head. Three months ago, periodical headache in occiput, violent in character and accompanied by vomiting, set in. At present vision is normal. A few weeks later, R. E., V = $\frac{1}{2}$; L. E., V = $\frac{1}{2}$. Choked disk on each side. Pupils of normal size, reacting well to light and movements of convergence. Five weeks later, total blindness of each eye. Swelling of the papillæ began now to decrease, and in the course of some months total atrophy of the optic nerves was observed. One year from the inception of the symptoms an epileptiform attack occurred. Then followed paresis of bladder and rectum, also of left upper and left lower extremities, paralysis of right facial nerve. Pupils equally dilated, and showing no reaction to ordinary stimuli; frequent epileptic attacks; finally death. Post-mortem showed an ivory exostosis from the inner surface of the skull-cap, embedded in the right lobe. In this position a heteroplastic mass was found, having been directly in contact with the exostosis. It proved to be a fibro-sarcoma. The dura mater was normal, pia mater turbid. Structure of cerebrum and cerebellum more or less anæmic: some serous effusion in the ventricles. The author attributes the early disturbance of vision primarily to the direct pressure of the tumor; later the increased intracranial pressure probably contributed. [Points of interest in this case are (a) the time which elapsed between the reception of the injury and the visual disturbance; in ordinary cases of brain tumors, of course, the exact time of commencement of growth cannot be so accurately determined; (b) the great rapidity of the destruction of the sight of both eyes; probably to be attributed to

the pressure of a distended third ventricle upon the chiasm (Türk), rather than to the direct pressure of the tumor. H.]

Case II.—A young and healthy man became suddenly affected with diplopia, due to paralysis of right rectus externus. Soon after, an attack of facial erysipelas was followed by paralysis of the left rectus internus. At the same time cerebral symptoms, headache, etc., were developed. The paralysis then rapidly extended to the right facial and acoustic nerves, and the branch of the third supplying the right inferior oblique muscle. Then the superior and inferior recti of the left eye became paralyzed. Left hemiplegia gradually developed; then paralysis of the hypoglossus, dysphagia, paresis of the vesical and rectal sphincters. The ophthalmoscope about this time showed slight swelling of the papilla, its outline obscured, retinal veins enlarged and tortuous, but no ecchymoses. Death seven months after the first symptoms noticed. Autopsy revealed marked fullness of the arachnoidal veins of the right hemisphere, both lateral ventricles filled with fluid, a gliosarcoma involving the right half of the pons.

The explanation of the development of the symptoms then was: the tumor started in the nucleus of the right abducens, affected the facial and acoustic, the nucleus of the right patheticus and that of the hypoglossal. Evidently up to this time the nuclei of the oculo-motorius were not disturbed by the direct pressure of the tumor, because of the different amount of paralysis of each third nerve; on the right side only the branch to the inferior oblique, while on the left side four distinct branches were involved. The explanation for the involvement of the oculo-motorius is to be sought in the connection which exists between the nucleus of the abducens and that of the oculo-motorius of the opposite side; the former being pressed upon, movement of the left eye towards the right was hindered; in other words, the paralysis of the left oculo-motorius was reflex. The author further suggests the possibility of a connection between the nucleus of the patheticus of one side and that of the oculo-motorius of the other side. This at present appears to be an open question. [The leading point in this case is what the author reports as an illustration of associated ocular paralysis. In other words, where two central nuclei of motor nerves

concerned in an associated movement have anatomical connection, the destruction of one nucleus impairs the function of the other. Thus, the rectus internus of one side and the rectus externus of the other are concerned in the lateral movements of the eye directly outward. Now, if the abducens nucleus, which is in anatomical connection with the motor oculi nucleus, be destroyed, not only will there be destroyed innervation of the abducens, but also of the rectus internus, at least in the lateral movement. A few cases of this kind have been reported (A. Graefe, Féréol). In Féréol's case, without paralysis of any other muscles, this associated movement was wanting. The diagnosis of a tubercular tumor situated in an abducens nucleus was made, and the post-mortem revealed a small tumor exactly in this position. This, however, may not pass without criticism. It is evident that direct pressure is not the only factor to be considered in the symptomatology of brain tumor. The effect produced upon the intracranial circulation and the general intracranial pressure, and the question whether the lesion be in the motor area of the cortex cerebri or somewhere between it and the nerves which it governs, must also be considered. H.]

Case III.—Mr. C., a lawyer, æt. 46, noticing difficulty in reading at night, applied for a prescription for glasses. On examination, L. V. very poor, eccentric fixation, only quantitative light-perception in the outer part of the visual field; R. V. good. Motility of each eyeball normal; media clear; pupillary movements normal under incident light. There was, however, bilateral myosis. Fundus of R. E. normal. In left fundus optic disk atrophic. Retinal arteries attenuated. Fifteen months later, left eye was completely amaurotic. R. V. had deteriorated; lower outer quadrant of the disk showed a white discoloration; diameters of pupil had increased to medium size. Five months later, R. V. had again markedly decreased, and the nasal half of the field had narrowed. The gray discoloration of disk was more marked and the arteries thinner. Still no general symptoms could be discovered. Knee-reflex was normal. The pupils were equally widely contracted upon accommodation effort, but responded very little to light. Seven months later, vision had decreased from $\frac{1}{2}$ to $\frac{1}{10}$; fixation had become eccen-

tric. One month later, there was complete amaurosis. Up to this time there were no general symptoms. By and by morbid mental symptoms appeared, such as irritability, querulousness, egotism, extravagant ideas about his wealth. Both optic disks were now of a pronounced gray, deeply excavated. Retinal arteries hardly discernible. Nystagmus set in, the movements of the eyeballs being partly rotary, partly vibratory. The patient saw most beautiful phosphenes. The *manie de grandeur* increased. The patient was removed to an asylum, and died suddenly a short time afterwards, being seized with convulsion followed by coma. Post-mortem showed a large quantity of bloody serum in the arachnoid space; pia mater adherent, especially over left frontal lobe; left frontal and parietal lobes flattened; right occipital lobe shorter than normal: cerebral substance very soft and oedematous; cerebellum not quite so soft. [This case, the reviewer supposes, should be classed among the psychical cerebropathies,* *i.e.*, those dependent upon a primary cerebral or cerebro-spinal lesion, to which the psychical symptoms add themselves as a complication; to be distinguished from the dementia paralytica. The case is an important one, as it was followed from beginning to end. Notice—1, the early appearance of the eye-trouble; 2, the rapidity with which the total atrophy sets in; 3, the manner in which the visual field is limited, central fixation being lost before the amaurosis appears; 4, the attenuation of the retinal arteries. The resemblance between the eye-symptoms in cases of this kind and in *tabes dorsalis* has been noted by observers. In this connection, the existence of normal knee-reflex is an interesting point. H.]—*Archives of Ophthalmology*, vol. xii. p. 364.

THE EYE-DISTURBANCES IN *TABES DORSALIS*.

Dr. L. Schmeichler, of Vienna, has lately written an interesting paper on this subject. The following leading points are abstracted from it. The clinical material upon which the paper is based was derived from the eye clinic and a hospital department partly devoted to nervous disorders; also from the Home for Incurables. As the eye-symptoms of *tabes* are generally initial, the cases were,

therefore, studied in the inception, in the progress, and in the decline of the disease. The eye-symptoms may be divided into—1, those of the optic nerve; 2, those of the pupil; 3, those of the eye muscles.

1. Disease of the optic nerve. Atrophy is a frequent and early symptom of *tabes*: hence the importance, in case of atrophy, of testing the tendon reflex, etc. Schmeichler has never seen a commencing optic nerve atrophy where the spinal sclerosis was fully developed, and estimates from his own experience and that of others that forty per cent. of cases of incipient optic nerve atrophy may be traced to *tabes*. Both eyes are affected at the same time, but often in different degrees. The only essential ophthalmoscopic appearance is the discoloration of the papillæ; this is the first symptom of the atrophic process, but as the disease advances vessel-changes are apparent. The apparent calibre of a vein on the papillæ may enlarge to double the normal size, retain the enlargement for some distance from the disk, and then gradually narrow to the normal calibre. This is independent of anything like papillitis. One cannot tell from the ophthalmoscopic appearances whether the visual acuteness is much decreased or not; papillæ which apparently are exactly similar are associated with very varied amounts of sight. The characteristic field of vision is the one with concentric limitation. 2. The pupillary changes in *tabes* are (a) reflex iridoplegia, or lack of pupillary reaction to light; (b) total iridoplegia, or absence of the reaction on convergence (the accommodation being intact); (c) myosis, or contracted pupil; with atropia it may be widened to four or five millimetres' diameter, and rapidly contracts again under pilocarpine: the pupil dilated with atropine, if left to itself, requires a long time (four to five weeks) to reach the same size it possessed before the use of the mydriatic; (d) inequality of the pupils, a striking symptom, but not so characteristic as those previously mentioned. 3. Changes in the innervation of the ocular muscles. This results in paresis of one or several of the muscles. It generally occurs very early in the disease,—sometimes precedes every other symptom by many years. A clinical illustration of this is given. The paresis generally comes on gradually; may remain constant, or after several months disappear entirely, in

* Vide Schtûle. Ziemssen's Encyclopædia, Zweite Deutsche Auflage, Bd. xvi.

some cases reappearing after several years.—*Archives of Ophthalmology*, vol. xii. p. 335.

NEURASTHENIC ASTHENOPIA.

Dr. Wilbrand, of Hamburg, employs this name to designate a well-known group of cases usually classified under the name of retinal anæsthesia or retinal hyperæsthesia. He thinks that it ought not to be considered as merely a local morbid condition, but as one of a series of morbid phenomena having their foundation in an abnormal state of the general nervous system. The first point to which attention is directed is that in these cases we find all grades of retinal anæsthesia. To illustrate this, the clinical histories of a number of cases are given, together with perimetric charts of the visual field. An interesting observation is reported in connection with these charts. If the field be taken by moving the test along the meridians, say from left to right, and, after the field is completed, the patient has rested twenty minutes, the examination be repeated, and the test be moved from right to left, it will be found that, while the general outline of the two fields is the same, the contracted portions are in opposite halves of the field. This is explained on the theory of fatigue of the retinal elements from the examination. This form of visual field defect is to be distinguished from another in which there is limitation of the field in all meridians. It is noteworthy that in cases with very pronounced limitation and apparently defective vision, the patients have no difficulty in finding their way, and from their movement alone no indication is afforded of the ocular disturbance. Another common phenomenon is that during the perimetric examination the test-object will disappear and reappear at short intervals. These symptoms, along with others, such as lack of ability to read for any length of time, symptoms of weariness on the part of the ocular muscles, point, in the author's opinion, to a neurasthenic state of the nerve-centres belonging to the eye, and also to an abnormal state of the whole nervous system. In considering the diagnosis of this affection, attention is directed to the prompt and extensive reaction of the pupil to incident light. As regards the etiology, the author thinks the cases may be divided into two groups: one which is the result of congenital tendencies, the other being

acquired. Masturbation is not to be considered a direct cause. Anæmia, chlorosis, chronic atrophic parametritis (Forster and Freund), concussion of the brain, are adduced as causes. References are made to Beard's opinion as to the part played by the vaso-motor system in the production of symptoms in neurasthenia, and the foundation of this opinion,—viz., the hyperæmia of the optic papilla.—*Archives of Ophthalmology*, vol. xii. p. 428.

INTRAOCULAR HEMORRHAGE A COMPLICATION OF CATARACT-EXTRACTION.

Warlomont reports the following case. The patient, a woman, æt. 63, was seized with vomiting following an unauthorized bodily effort. The extraction had been previously made, unattended by any abnormal occurrence. Following the vomiting an intraocular hemorrhage occurred, violent enough to force its way through the cut and the bandages. On removal of the latter a large coagulum was found in the cut, widely separating its lips. Symptoms of panophthalmitis followed, and the ball was enucleated. Examination of the ball showed that the hemorrhage originated in the choroid. The vessels of the uveal tract were turgid. Retina and choroid were separated. Three months later, the second eye was operated on for cataract by extraction. This operation was also unaccompanied by any special difficulty, but a short time afterwards hemorrhage likewise occurred, followed by the ruin of the ball.—*Centralblatt f. Augenhk.*, December, 1883. [The point of interest in this case is the destruction of each eye by the same cause,—viz., secondary hemorrhage. This probably had its primary cause in some part of the motor apparatus of the blood. The age of the patient suggests sclerosis of the arterial coats. This case recalls certain cases of glaucoma where secondary hemorrhage occurs after iridectomy, and which, in the reviewer's opinion, are to be explained by sclerosis of the intracranial carotid. A practical question in this connection is whether it is proper to perform an iridectomy operation on the second eye when the same operation on the first has resulted disastrously in the manner described. The reviewer thinks not, so long as any other method offers a fair chance. In the case of cataract-extraction some of the extractions without iridectomy would be preferable to

the Graefe method. In glaucoma it cannot be said that any therapeutic measure has yet been devised equal to the iridectomy, but in such cases as those referred to above the reviewer thinks that it is contra-indicated.* H.)

AN OPHTHALMIC OBSERVATION BY THE KOCH CHOLERA COMMISSION.

The following note occurs in the report of the Egyptian Cholera Commission: "Further, about fifty patients suffering with the Egyptian ophthalmia were examined. It was found that this name is applied to two different morbid processes. The one pursues a malignant course, and is caused by bacteria resembling the gonorrhoeal micrococci, with which they are very probably identical. In the second and less dangerous process very small bacilli are constantly found among the pus-corpuses (in den Eiter-Körpchen).—*Centralblatt für Augenheilk.*, December, 1883. [The term "Ophthalmia Ægyptiaca" is one of the names which has been applied to the disease commonly known as granular lids. It originated about the beginning of the century in connection with the furious epidemic of ophthalmia which assailed the French and English troops in Egypt. H.]

CATARACT-DISSECTION FOLLOWED BY DEATH.

Rheindorf reports a case of a five-month-old healthy child with congenital cataract upon whom he performed a dissection. No bad symptom followed for fifteen hours, when convulsions and death occurred. The probable cause, the author thinks, was an intense excitation of the ciliary nerves from the swollen lens producing cerebral congestion. [*Post hoc or propter hoc?* H.]—*Klinische Monatsblätter*, December, 1883.

MONOCULAR CATARACT COMBINED WITH RACHITIC DIATHESIS.

Dr. Heuse reports the following case: The patient, æt. 4 years, had a marked rachitic projection from the right half of the forehead, which had first been noticed three years before, with other symptoms of rachitis. The right lens was partially opaque, the opacity being composed of a central point in the neighborhood of the posterior pole, with four short radiating prolongations. Unconnected with this

was a slight diffuse clouding of the lens-tissue. As the history of this case showed that some of the rachitic changes had already disappeared at the time of the above observation, the author thinks that possibly many cases of monocular cataract, when no bone-affection is demonstrable, may at some time have suffered from rachitis.—*Centralblatt für Augenheilk.*, December, 1883.

A NEW REFLECTING-MATERIAL FOR MIRRORS.

Schoeler directs the attention of the profession to a new material for coating mirrors which has been discovered by Mr. Lohmann, of Berlin. It is a platinum preparation, and glass mirrors coated with it possess the property of simultaneously reflecting and allowing rays of light to pass through the mirror. The color of the coated glass resembles somewhat that of the smoked glass of the opticians. The reflecting powers of these mirrors as yet have not been investigated. Perhaps there may be some useful application made of this material in the construction of ophthalmoscopic mirrors.—*Klinische Monatsblätter*, December, 1883.

ON THE EARLY MANAGEMENT OF INFANTILE PARALYSIS.

BY G. BETTON MASSEY, M.D.,

Electro-Therapeutist to the Philadelphia Orthopedic Hospital and Infirmary for Nervous Diseases.

Read before the Philadelphia Clinical Society, February 22, 1884.

GENTLEMEN,—The purpose of this paper is to call your attention to the importance of an early diagnosis of infantile palsy, and to point out the most approved methods for its therapeutical management. Yet, since I am not addressing a body of specialists, it may be well to anticipate these topics by presenting to you an outline of the disease itself as observed clinically.

Under the name of infantile paralysis or palsy, or, as it has been pathologically designated, acute anterior poliomyelitis, we must not make the mistake of including all forms of paralysis that occur in infancy. This period of life is liable to many forms of paretic disturbances of motility and sensation, such as those due to peripheral nerve injuries, to sclerotic changes in the central nervous system, and to altered conditions of blood-supply to

* In this connection, vide A Contribution to the Operative Treatment of Glaucoma, by Albert G. Heyl, M.D., Trans. Amer. Ophth. Society, 1883.

the brain resulting in hemiplegia. In fact, the last form of palsy, presenting all the characters of hemiplegia in the adult, is by no means uncommon in children of tuberculous parents, and requires to be carefully separated from the disease under consideration.

A typical case of true infantile palsy in the second stage—the stage in which it usually comes under observation in hospitals—is briefly characterized by the following salient conditions: wasting and weakness of the anterior tibial and peroneal groups of muscles of one or both legs, shown functionally by a flapping of the foot in walking, and inability to flex or peronate it; wasting and weakness of the quadriceps extensor muscle of one or both sides, with imperfect extension of the knee; loss of faradic response in muscles affected; coldness of the paralyzed extremity; and probably arrested growth of the whole limb or limbs. I have spoken here in the plural, but usually we find but one limb affected in this stage.

Such is an enumeration of the features invariably found in some degree in all cases of typical infantile paralysis subsequent to the first few weeks of the attack. To the picture may be added the distortion that is apt to be occasioned by the unopposed action of the healthy muscles of the part producing a form of club-foot; the arrested growth of the whole limb as compared with its fellow; and a statement that sensation remains perfect,—unfortunately for certain plans of treatment.

It is, however, to the first stage and its diagnosis that I wish to call your especial attention to-night,—the initial act in the drama that is destined to leave so great a mark on the life of the young individual. It is here that the work of the general practitioner touches closest on the disease; and on his skilful use of vigorous remedies much responsibility rests. The child, from one to three or five years of age, in perfect health, is suddenly attacked with intense fever. This fever may be accompanied by convulsions or other cerebral symptoms, but is almost always merely associated with serious derangements of the digestive apparatus. The paralysis follows with equal suddenness, usually gaining its greatest height between night and morning. The amount of the paralysis differs much in different cases, but is always far greater than the amount that will be found

to be present a few days afterwards, when the causative congestion of the cord has subsided. Usually both legs will be powerless; sometimes merely the deltoid muscle of one side; while in other cases all the muscles of the body are equally powerless, the patient being even unable to lift the head. Notwithstanding this extensive loss of motility, the sensation throughout the body remains perfectly normal, and the automatic integrity of the bladder and rectum beyond reproach. The diagnosis of the acute attack may be summed up under four points: viz., 1st, sudden motor paralysis, usually of one or both legs, subsequent to fever,—possibly in its absence; 2d, absence of any disturbance of sensibility; 3d, absence of paralysis of bladder or rectum; 4th, absence of marked cerebral disturbance other than that due to the febrile movement.

The pathological processes involved in the production of this acute stage are highly interesting and afford important suggestions for treatment. All observers are now practically unanimous in placing the real seat of mischief in the anterior horns of gray matter of the spinal cord, and are even still more exact in confining the location strictly to the large motor cells that are found in this situation. Although ocular demonstration of their condition in the very early stages is yet wanting, it is certain that the changes commence in the cells themselves and not in the surrounding neuroglia, and that the changes are of an irritative character,—a positive inflammation of the cell-elements in a minute area of the anterior cornu. This inflamed area may be of considerable extent longitudinally, but is strictly confined to the system of large motor cells,—in most cases even being confined to less than the whole of these cells, as the immunity of certain muscles from atrophy will show. The hyperæmia, however, that accompanies the more positive inflammation of certain cells nearly always involves the whole of this cell-group, so that we always have a greater paralysis at first than will be found to persist subsequently. In fact, a peculiarity of this trouble is the steadiness with which the paralytic symptoms will recede during the first few weeks of the attack, giving rise to false hopes of ultimate and complete recovery in the minds of anxious parents and too expectant physicians. The unvarnished truth is that

unassisted recovery will proceed only so far as the disappearance of symptoms due to the congestion surrounding the inflamed cells, while within that circumscribed area a blow has been struck that is beyond the reparative powers of nature.

What, then, is the patho-physiological expression of this change in the great cells of the anterior cornua? Simply an interference, or more or less complete abolition of their functions: as these functions are motor and trophic, we have an immediate motor paralysis of the muscles supplied by this area, together with an active degenerative process in the muscular tissue. The muscles not only cease to act, but tend rapidly to waste away by a process of fatty degeneration exactly similar to the retrograde metamorphosis that occurs in muscular tissue when it is severed from its nutritive centre by disturbance of nerve-conduction.

The electrical reactions are a most delicate test of this secondary change in the muscles, and by them we can predict the impending atrophy with absolute precision long before it becomes evident to the eye. They consist of the peculiar series of changes that are found alike in this disease, in lead-palsy, and in peripheral palsies due to nerve-injury, and that have been denominated the "reaction of degeneration." Briefly stated, this reaction of degeneration consists of a failure of the faradic current to produce any response in the muscle; increased action of the muscle to the galvanic interruption stimulus; and a relatively greater action from the anode than from the kathode. It will be seen, later, that these departures from the ordinary electrical responses have an important bearing upon treatment as well as upon diagnosis.

Strangely enough, this disease has been found occasionally to occur also in the adult individual, although the very great majority of cases are in children from eight months to five years of age. These instances of adult poliomyelitis do not differ in any essential particulars from the type here given.

Having properly diagnosed the case, what should be done by the physician to relieve the little sufferer and lessen, if possible, the threatened deformity? If he is fortunate enough to be called in at the outset, he should institute without delay an active antiphlogistic treatment, directed

to the seat of the disease in the cord. This should consist of blisters on the back, over the lower dorsal and upper lumbar vertebræ, if the case be the usual one of paralysis of the lower limbs; higher up, at the nape of the neck, if the shoulder or arm is affected; revulsives to the extremities; low diet and appropriate constitutional treatment. No electrical applications should be used at this time, except possibly for purposes of diagnosis, since the condition is one of acute irritation. Such would be the proper treatment for the first four or five days. After that time, and when the blister has healed, gentle, continuous currents (galvanic) of two or three minutes' duration should be applied to the back, and the regular galvanic applications to the muscles be begun. These should consist of the galvanic current so interrupted as to produce contractions when the poles are applied close together on the muscles. No more current should be used than is necessary to obtain motion; and, since the reaction of degeneration is always present in the early stages, it will be found that placing the anode above will insure the greatest activity of the muscle with least pain. It is by no means necessary to make each muscle contract separately, but careful attention should be bestowed upon the physiological groupings of muscles, in order that those that are naturally associated in movement should be simultaneously acted upon.

As I said before, these applications should be made with the galvanic form of electricity only; and I cannot here too strongly condemn the usual prescription of faradism for such cases as unscientific and totally useless. Even if galvanism fails to produce contraction, the well-known electrolytic and catalytic effects of this current make it of great service to the paralyzed muscles.

The sittings should be repeated thrice weekly for months at a time, and should be alternated with thorough professional massage if possible. Under such a régime, and with the prescription of small doses of strychnia in late stages, it is certain that the paralyzed member can be kept nearer to the condition of its more quickly growing fellow by persistent and careful treatment. If neglected during the first years, the loss of time is irreparable.

CHLOROFORM SHOULD BE USED IN STRYCHNIA-POISONING—NOT TOBACCO.

BY FRANCIS L. HAYNES, M.D.

IN a recent number of this journal a case of strychnia-poisoning treated by infusion of tobacco is recorded (*Phila. Med. Times*, February 23, 1884, p. 407).

It has been well established, by published cases and experiments, that tobacco, though a powerful emetic, is not a true antagonist to strychnia. It should not be used in strychnia-poisoning, for the following reasons:

1. Tobacco, taken by human beings, may produce convulsions and death.
2. Nicotin produces convulsions in the lower animals.
3. Animals that have recovered from doses of nicotin and strychnia given at separate periods, may be killed by the same doses given together; showing that they aid, instead of antagonizing, each other.
4. Chloroform and chloral are certainly the most efficient remedies yet proposed for strychnia-poisoning. In severe cases, where it is impossible, on account of the firm contraction of the muscles of the jaws, to use internal medication, chloroform may be used by inhalation, or chloral by enema, where otherwise the physician would be powerless. Chloroform, in such severe cases, is by far the best remedy, as chloral is very apt to be rejected by the rectum.

REFERENCES.

Cases of convulsions produced by tobacco: 1. Stillé, *Therap.*, Phila., 1868, ii. 325 *et seq.*, mentions five cases, some ending fatally. 2. Mitchell, *Therap.*, Phila., 1850, 544, quotes a fatal case from *London Lancet*, March, 1850.

Experiments with nicotin and strychnia: 1. Reese, *Amer. Jour. Med. Sci.*, April, 1871, 382. 2. Wormley, *Micro-Chemistry of Poisons*, New York, 1865, 545. 3. Haynes, pamphlet, from *Trans. Am. Philosoph. Soc.*, March 16, 1877.

Chloroform in strychnia-poisoning: 1. Dresbach, Tiffin, O., said to be the first who gave chloroform internally for this purpose. *Case*.—A man swallowed three grains of strychnia in solution; very violent symptoms in twenty minutes; two drachms of chloroform swallowed; complete relief in less than twenty minutes. *Am. Jour. Med. Sci.*, April, 1850, 346; Wormley, *op. cit.*, 544. 2. Leech, *Med. Times and Gaz.*, November, 1863, 487. 3. Smith, J. R., *Am. Jour. Med. Sci.*, July, 1860, 278. 4. Hly, recovery after four grains of strychnia. *N. Y. Journ. of Med.*, 1859, 422. 5. Gobrecht, *Antagonistic Effects of Chloroform on Strychnia*, *Trans. Penna. State Med. Soc.*, 1871.

Chloral in strychnia-poisoning—experiments. Bennet, *Antagonisms of Medicines*, London, Churchill, 1875.

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PROF. HENRY G. LANDIS, of Columbus, Ohio, delivered an address on the "Need of Culture in the Physician" before the Alumni Association of the Jefferson Medical College.

TRANSLATIONS.

CHOREA OF THE LARYNX.—According to Blachez, chorea of the larynx in children is of the rarest occurrence, though a similar affection is often met with in hysterical women. Blachez has seen two cases of this disease in children.

The first case was that of a lymphatic child of arthritic parents, who at first suffered from a slight laryngitis with spasmodic cough, which in a few days assumed a most remarkable character: five or six violent coughs rapidly followed one another, accompanied by a hoarse barking, which, however, did not interfere with sleep. No other choreic symptoms were present. Belladonna and valerian were employed without effect, though the attacks, which at first were very frequent and of short duration, became of rarer occurrence, though their duration was prolonged for one or two hours. Chloral finally arrested the attacks.

The second child was weak and anæmic, and in the course of a slight bronchitis became subject to attacks of spasmodic coughing, which were accompanied by a peculiar cry which most closely resembled the sound made by a mechanical toy sheep. This cry, which was always of the same character, occurred nine or ten times within a few minutes, the attacks coming on several times during the day, but never during sleep.

The child is very nervous and excitable, and constantly covered with perspiration; no choreic movements are present. The disease disappeared in about three weeks, under the use of bromide of potassium.—*Centralbl. f. d. Med. Wissen.*, January 5, 1884. S.

PTOMAÏNES.—L. Giacomelli reports (*Lo Sperimentale*, October, 1883) the discovery of a cadaveric alkaloid which in its principal reaction closely resembles picrotoxin. By the investigations of Maas, ptomaïnes were obtained resembling strychnia and morphia in their effects upon animals.—*Fortschritte der Medicin*, xv., 1883; from *Deutsche Medizinische Zeitung*.

LEMONADE FOR DIARRHŒA.—Dr. Vigouroux recommends a glass of hot lemonade every hour, or half-hour, as an easy, agreeable, and efficient treatment for diarrhœa.

PHILADELPHIA
MEDICAL TIMES.

PHILADELPHIA, APRIL 5, 1884.

EDITORIAL.

CRITICISMS UPON THE MICROBIC
THEORY OF THE ORIGIN OF
TUBERCLE.

WITH the present issue we lay before our readers the conclusion of Dr. Formad's contribution to the controversy regarding the origin of tuberculosis. We have felt warranted in devoting an unusual amount of space to the publication of this important article in full, because we regard it as embodying the most able criticism of the specific bacillary theory of Koch that has yet appeared; and also because an abstract of the argument of Dr. Formad would not have done him justice. We hope that it will be read carefully by all who are interested in this most important topic. In order to make the discussion more complete, the results of Koch's latest investigations, given in his report to the Imperial Board of Health, will be presented in our next issue.

It is evident that the demonstration of the existence of a microbe justly entitled by its specific and contagious properties to be designated as the *bacillus tuberculosis*, requires a concurrence of expert testimony in its favor, from skilful and competent observers in the domain of mycology, in that of pathological anatomy and of clinical medicine. It would be unnecessary to say that unless the theory which is offered agree with the known facts of morbid anatomy and clinical observation, it cannot be sustained,—no matter how plausible it may appear or how complete the chain of reasoning may be in the direction of microphytic investigation,—were it not that there seems to be some danger that clinical observers may overlook the

fact that Koch's studies do not at all affect our knowledge of the symptomatology of phthisis: it is just as contagious—no more and no less—as it was before the specific *bacillus tuberculosis* was reported. Previous to the announcement of Koch's discovery, clinical teachers in Europe and America were almost unanimous in their opinion that phthisis was not contagious; since the promulgation of Koch's theory that "consumption is caused by inoculation with the *bacillus tuberculosis*, and in no other way," however, there has occurred almost a revolution in medical teaching, and clinicians are vying with one another in bringing forward evidence of the contagiousness of tuberculosis. Just as we had, as we thought, definitely settled the diagnosis of tubercle upon an anatomico-pathological basis (the essential properties being (1) an aggregation of small cells, with giant-cells frequently, if not always; (2) an intercellular net-work, and (3) a tendency to retrogressive metamorphosis), we are asked to subscribe to a new criterion of tubercle, the presence of the *bacillus tuberculosis*; everything is tubercle which contains these bacilli, nothing is tubercle where they are absent; giant-cells are merely swollen cells containing bacteria as in a capsule. This criterion is indeed new; clinical observers and pathologists are now busily engaged in determining whether or not it is also true. From these laborious and precise histological and clinical investigations great advantage will undoubtedly result, even if the Koch hypothesis be tried and found wanting.

Already some of Koch's followers are prepared to admit that the *bacillus tuberculosis* requires a suitable soil for its development and growth: what this antecedent something may be is still undefined; but it is evident that it cannot be very different from the tuberculous dyscrasia so long and so well known to pathologists and physicians.

It is a general law in pathology that morbid processes tend to self-perpetuation; moreover, a morbid reaction existing in any particular tissue tends especially to extend in tissues and organs of like structure. Therefore, under favorable circumstances, all diseased processes may become infective.

It has been suggested that the cultivation and inoculation experiments with infected material may be simply an evidence that the bacilli are excellent carriers of infection, and do not readily part with this power when it has been once acquired. Their peculiar chemical characters are easily accounted for by their habitat. It is also known that the several varieties of microbes are not permanent in their morphological characters, but are capable of developing into different forms under varying conditions of culture. (Naegelle.)

Furthermore, from the experiments of Tyndall, we are satisfied of the almost universal presence of spores in the atmospheric air which are capable of causing the different varieties of fermentation and putrefaction by their development and multiplication. These germs of disease, as they have been called, may really be humble ministers of health, since they attack highly-organized devitalized or dead tissues and reduce them to simpler chemical compounds. It may be that these ordinary germs find a suitable soil for growth in the tissues of low vitality in the consumptive patient, and may then *secondarily* cause disturbance and decay by their development. The patient, under such circumstances, would not be tuberculous because he was the host of the bacilli, but just on account of the previous existence of the tuberculous dyscrasia would his tissues form a proper soil for the germs. Tuberculosis then would resolve itself into a peculiar necrobiosis, and the principal object of treatment would be not to destroy the microbes, but to labor so to build up the powers of nutrition as to

render the patient an unfavorable soil for their detention and development. Clinical experience supports this view of the nature of phthisis pulmonalis, since all over the world hygienic and restorative treatment has produced better results than can be hoped for from the use of antiseptic medication.

A "RECLAMATION."

THE *Moniteur de la Polyclinique*, in an article "aux Yankees," is very complimentary to us, but, consistently with French character, is a trifle inexact. It accuses the Americans of not reading the *Moniteur*, but, in the next sentence, claims that we plagiarize shamelessly. This is certainly a compliment to our intelligence, for we must be bright indeed if we can plagiarize without reading the original! As an illustration of the truth of the statement, the *Moniteur* cites an article on "Epilepsy," which Dr. W. Thornton Parker (who is spoken of as the redacteur (!) of the *Philadelphia Medical Times*) was supposed to have written, but which the editor of the *Moniteur* claims is merely a paraphrase of an essay which he wrote upon the same subject in 1875. We find, upon reference to Dr. Parker's article on "The Treatment of Epilepsy" (p. 892, vol. xiii.), that it is only two pages in length, that it disavows novelty at the outset, and that it is made up of the reports of two cases from Dr. Parker's private practice, with one or two quotations from current literature and standard authorities. Unless the editor of the *Moniteur* has a copyright upon all that is known up to the present time about epilepsy, we cannot understand the force of his criticism, if that article be the one referred to. By the way, will not our metropolitan contemporary, the *Medical Record*, kindly inform the editor of the *Moniteur* as to the personality of the "redacteur" of this journal, and so relieve his mind of some evident uncertainty upon the subject?

NOTES FROM SPECIAL CORRESPONDENTS.

CINCINNATI.

COLLEGE COMMENCEMENTS.

CINCINNATI medical colleges have been more poorly attended during the last year than for a number of years before. The result has been a greatly-decreased number of graduates from most of the schools.

The commencement exercises of the Cincinnati College of Medicine and Surgery were the first to be held. They graduated a class of fifteen, and Dr. Amick delivered the valedictory. Next came the Miami Medical College, with twenty-eight graduates, and Dr. William H. Taylor as valedictorian. The Ohio Medical concluded the list, with a class of one hundred. Prof. P. S. Conner delivered the valedictory. The attendance at the irregular schools has suffered even a greater decrease. The Pulte Medical College (Homœopathic) graduated a class of sixteen, when they usually have twice the number, in point of numbers it ranking, as a rule, with the Miami.

ANATOMICAL MATERIAL.

Considerable excitement has recently been created in our city by a murder perpetrated for the sole purpose of selling the bodies for dissecting purposes. The laws of Ohio permit the bodies of unclaimed paupers to be given to the medical colleges. This would furnish plenty of material, but for the fact that these bodies are not unfrequently claimed by people who manifested no interest in the individual while living. The result has been some shortness in anatomical material, which Dr. Cilley, demonstrator at the Ohio Medical College, has endeavored to remedy by calling into service two resurrectionists of this city. Some three weeks ago, it appears, these two worthies—colored men, named Ingalls and Johnson—thought that they could more easily knock down an aged colored couple and a young girl who lived with them, than resurrect some bodies already dead and buried. They knew well the lonely habitation of the old darkies, and were without any doubt as to the result of their expedition. So sure were they of success that they even made the bargain for the delivery of some "stiffs" with Dr. Cilley and engaged an express-wagon to haul them in while their intended victims were still in the innocent enjoyment of their usual health. These two fiends went to the home of the intended victims, knocked them on the head, and when the express-man arrived the subjects were ready,—still warm, it is true, but dead beyond a doubt. After they were delivered to the College the murderers returned and set fire to the hut, hoping that all evi-

dence of their crime might thus be destroyed. However, one thing after another pointed to the commission of a horrible crime, until the authorities began to search for the bodies. After not a long while, these were found in the Ohio Medical College, awaiting the opening of the spring session. The secular press has been filled with sensational accounts of the tragedy, and much blame has been cast upon the profession in general and Dr. Cilley in particular.

Notwithstanding Dr. Cilley's ready aid in the discovery of the bodies (and without his aid they would never have been discovered), the daily papers have vied with each other in their endeavor to cast the most suspicion upon him. The profession, as a rule, have stood by one another. One physician appeared in print in a medical journal of our city seeming to insinuate that Dr. C. was not altogether blameless in the matter, but he explained in the next number of the same paper that he did not intend what his language seemed to imply.

A. B. T.

March 14, 1884.

CHICAGO.

CHICAGO has, all told,—State, county, and United States,—thirteen hospitals. The whole number of beds, including the United States Marine Hospital, is one thousand one hundred and ninety-one. There is now in process of completion accommodation for an additional two hundred and fifty beds, which will give us, at the end of the year, a total of one thousand four hundred and forty-one. Many of the institutions herein included occupy buildings constructed without due regard for sanitation, now considered so necessary for the better accomplishment of their work. There are a few, however, of recent construction, having all modern improvements, and among those may be classed the largest. The county hospital is the only one devoted to charitable work in its broadest sense; the others exact payment from nearly all patients. The fifteen hundred medical students in this city have regular instruction in four of the hospitals only, thus giving a very limited field of observation for so large a number of pupils.

The forty-first annual commencement exercises of Rush Medical College took place at Central Music Hall, February 19, with a graduating class of one hundred and sixty-seven members. The banquet was given at the Grand Pacific Hotel in the evening, Professor James Nevins Hyde unveiling a portrait of Professor Allen, the occasion being the twenty-fifth anniversary of Professor Allen's connection with the school. The portrait is to be placed upon the walls of the main amphitheatre of the college.

The College of Physicians and Surgeons—

the latest candidate for patronage—opened this winter with a class of one hundred and seventy-four. The commencement exercises of this school were held at the Grand Opera-House, March 11, in the presence of the usual large audience. The Secretary opened the exercises with his report, in which he claimed that this school offered such advantages as to draw forty-two students from other colleges. He also claimed for the class a standard of literary attainments far above those of most students of medicine, and that the thoroughness of the preliminary examination for matriculation had deterred numbers from an attempt to enter the college. You will observe that our youngest is not to be outdone by the criers of other wares.

The commencement exercises of Chicago Medical College will take place March 25, with a graduating-class numbering fifty-two.

All our schools now claim to have reached a degree of excellence never before attained, mainly in the manner of teaching and examination. Though stress is laid upon the requirements and the strictness of the examination for matriculation, this is, in fact, the weak point. Few, if any, presenting for matriculation are rejected, and when a person is rejected upon this ground he must be densely ignorant.

In the doctorate address delivered at one of our college commencements, the speaker, in referring to the action of the county board in making appointments upon the medical staff of the county hospital, said, "You who are familiar with the political formulas of this city need not be told how these appointments are made. In your imagination you are present at that important meeting of the county commissioners. You recognize some of them, at least, whose occupations are not calculated to give an enlightened view of the wants of a large hospital or to develop a heart keenly alive to the want and wretchedness of the poor and distressed in a great city. They proceed to business: Commissioner Hans Olson nominates his friend Hans Jansen, M.D.; Commissioner Patrick Murphy nominates M. O'Flannigan, M.D.; Commissioner Bingenheimer nominates Dr. Ernst Schlie-senger. They are unanimously elected, and the most important interests of that magnificent hospital are handed over to the supervision of these men, who, however fully they may represent the different races of our body politic, or the political influence of a ward upon the county elections, in no sense represent the best medical talent of this great city."

Our local medical societies, two in number, languish; the meetings are thinly attended, the papers, as a rule, not being able, the discussions spiritless. This condition of things has existed for some time, and the meetings would, were it not for the younger element, degenerate into a mere social chat.

The law requiring physicians to return certificates of birth to the county clerk has never been generally observed, nor has there been any effort on the part of the authorities to enforce it, and the result is that physicians generally pay little attention to it,—possibly not more than one-third of the births being recorded. It appears that this laxity on the part of medical men is due to the difficulty and trouble in procuring the necessary blanks. Many physicians have applied to the city health department for the forms, and have there been informed that application should be made to the county clerk, in another part of the city. Applicants have, upon visiting the county clerk's office, found that they could not always receive prompt attention, and have gone away disgusted. A few physicians have addressed the county clerk through the mail, requesting forms to be forwarded, but without success; and now the authorities threaten to prosecute the delinquents.

A prominent practitioner of this city gave notice to the health commissioner a few months ago that he would not report cases of contagious disease to the health department unless compensated for such service, and recently the commissioner received a bill from the physician in question, demanding twenty dollars for twenty cases reported. The bill was referred to the city attorney for an opinion, and the claim was promptly denied in a note severely criticising the gentleman who thus refused to serve the city without compensation. The profession generally feel that this sort of work should be paid for by the corporation for which it is done, and that such service cannot in any sense be considered in the light of charity.

The State Board of Health, at a special meeting, considered the application of a number of physicians for certificates. Several were refused on the ground of unprofessional conduct, among them being one Dr. Simon M. Landis, a graduate of the Pennsylvania Eclectic College. This doctor claimed that he had mystic power, challenging clergymen and physicians to discussion, asserting that their teachings were unnatural, etc.

Notwithstanding the winter has been one of the most severe ever known, the health of the city has remained very good. Scarlet fever has prevailed to some extent, but the disease has generally been mild. M.

March 15, 1884.

SCARLATINA IN THE HORSE COMMUNICATED TO MAN.—Dr. Fenner, D.V.S., having slightly abraded his hand in giving a bolus to a horse suffering with what was considered equine scarlatina, was himself attacked, seven days later, by fever and a typical scarlatinal angina, which kept him in his room for a fortnight.—*New York Medical Record.*

PROCEEDINGS OF SOCIETIES.

NEW YORK ACADEMY OF MEDICINE.

A STATED meeting was held March 21, 1884, FORDYCE BARKER, M.D., LL.D., President, in the chair.

Dr. LEONARD WEBER read the scientific paper of the evening, on

LOCOMOTOR ATAXIA AND SYPHILIS.

The author reviewed the general evidence concerning a possible connection between locomotor ataxia and syphilis. The percentage of cases of locomotor ataxia in which syphilis was present, according to various authors, varied greatly; commencing from below 18 per cent., we have 18, 20, 21, 70, 80, 88, and up as high as 91 per cent. The latter two numbers are those of Erb. Some degree of variation in statistical figures could easily be accounted for, but such as existed here threw doubt upon the value of this method in determining the etiological relation of syphilis to posterior spinal sclerosis. Pathological observations had not definitely proved this relationship. Dr. Weber thought it not improbable that in constitutional syphilis the irritation of the syphilitic poison might start such an inflammation as would lead to the lesions which had been found in posterior spinal sclerosis when other conditions were favorable for its development. In his own practice he had found evidence of syphilis being present in fewer cases of locomotor ataxia as a possible etiological factor than several other conditions. The most common was rheumatism. Masturbation in early youth and continued for some time exerted a very baneful influence upon the nervous system in both boys and girls, and laid the foundation not only for so-called nervous affections, but for organic disease. The author had had a few cases of locomotor ataxia in the history of which masturbation in early youth was found to have been the foremost cause. Spinal tabes from a pathological and a clinical stand-point was rather a protean form of disease. So far as his own observations went in those cases in which there was also a history of syphilis, benefit was derived from anti-syphilitic treatment, and in some cases the benefit was quite marked, but there had not been in any case a complete cure. Other authors, however, had reported a few cases in which the symptoms entirely disappeared under anti-syphilitic treatment. In cases in which a syphilitic history was present, prognosis so far as some degree of improvement in the symptoms was concerned was usually better than in other cases, but the therapeutic results, even in such cases, had fallen far short of the anticipations of the positivists.

Dr. Weber's experience with syphilis had convinced him of the importance of taking

the disease early in hand, and of following up the treatment persistently for years, if the individual wished to escape the tertiary effects of the disease, and among others the baneful influence which it often had upon the cerebro-spinal system. When a syphilitic history was present in a case of tabes, mercury should not be given by the mouth, but by inunction, the latter method being far more efficacious and speedy in its action. The following statistics were given, based upon his experience with syphilis. The whole number of cases treated was one hundred and thirty-four, of which number one hundred and fifteen were men, and nineteen women. None of them were under observation less than four years, and most of them eighteen to twenty years. The details of one case were given, the only case he had ever seen in which there was a distinct history of syphilis occurring twice in the same individual. The last initial lesion appeared five years after the first. Of the one hundred and thirty-four cases, eighteen—that is, 13.5 per cent.—had up to the present time presented symptoms of specific lesions of the cerebral nervous system; eight of the eighteen, of the brain alone. Six of the eight were men, two were women. Five cases were of the brain and spinal cord combined,—all men; five of the cord only,—four men and one woman. Two of the eight cases which had syphilis of the brain alone, died of cerebral symptoms. The other six were still alive, but not cured. Of the five cases of syphilitic affection of the cerebro-spinal axis, two succumbed to the disease; three were still alive. Of the five with spinal syphilis, one, a woman, died of syphilitic paraplegia; four were living, and were relieved after treatment.

Of tabes he had records of seventeen cases. The first case was a typical one of locomotor ataxia. There was no syphilitic history. The second case was a typical case of tabes involving the brain; in the history was venereal excess. Syphilis suspected. In the third case were rheumatic influences, but absence of syphilis. In the fourth case there was typical tabes, and absence of syphilis. In the fifth case the patient contracted syphilis after having had tabetic symptoms many years. Rheumatic influences were the most important in the etiology. In the sixth, a typical case of tabes, there had been venereal excess; syphilis suspected; rheumatic influences. The seventh was an atypical case, with a history of rheumatic influences, but not of syphilis. The eighth was an atypical case, early masturbation a presumable cause; no syphilis. The ninth, a typical case, died of complicating cardiac disease. Cause, early and long-continued masturbation; rheumatic influences; no syphilis. The tenth, of spinal type, cause venereal excess; no syphilis. The eleventh, a typical form, spinal type; rheumatic influences; no syphilis. The twelfth, a typical case, no syphilis; probably

venereal excess. The thirteenth, atypical case of lumbar type; syphilis played the most important, perhaps the only, rôle; improved promptly under anti-syphilitic treatment. The fourteenth, atypical case, nothing but rheumatic influences. Fifteenth, slowly progressive case of typical lumbar type; syphilis twenty years ago; venereal excess for many years. Improvement with galvanism and hydropathy. Sixteenth, typical case of tabes; early masturbation; later, long physical exercise; no syphilis. Improvement with galvanism and hydropathy. Seventeenth, a typical case of tabes, involving the entire cerebro-spinal system, terminating in dementia paralytica, of which the patient died. Undoubted history of syphilis.

The author drew the following conclusions:

First. There was not sufficient evidence to show that syphilis might be the direct cause of a typical form of locomotor ataxia,—that is, of posterior sclerosis of the cord.

Second. There was proof, and plenty of it, that syphilis produced certain lesions in the cord and its meninges as surely, if not as frequently, as in the brain, and that these lesions might be, and often were, followed by symptoms of tabes, and that they were generally relieved by prompt and energetic specific treatment, but rarely cured.

Third. Experience had shown him that the tendency of the syphilitic virus to produce lesions in the nervous centres occurred the sooner the less its course was interfered with by judicious and long-continued anti-syphilitic treatment. Older cases were more apt to develop neuroses than those of recent date.

Fourth. As shown by all observers, after syphilitic lesions of the cerebral nervous system were once established, they might often be relieved, but seldom, if ever, cured, by specific measures. Thus, we had reasons in cases of syphilis for insisting upon long-continued and timely treatment, and to keep accurate and judicious watch over the patient, and have specific symptoms attended to as early as possible.

Fifth. The inunction treatment with mercury in fresh cases of syphilis was the best means of reducing the disease to an early and harmless latency.

The PRESIDENT said that, for the purpose of bringing out some remarks on certain points, he would venture to detain the audience a few minutes by referring to three cases which had come under his care, and which were very vividly brought before his mind by the paper of the evening. Although the author had inferentially shown the value of therapeutic results in throwing light on the pathology, he had not emphasized this point in such a way as to the President's mind it had been emphasized by personal observation, especially in another class of disease. In brief terms he would mention three cases in which the therapeutic results had led him to believe,

and to feel a strong conviction, that the locomotor ataxia was the result primarily of syphilitic poison in the system.

The first case occurred in 1873, in the person of a young man, unmarried at that time, who sent for him on account of what he called rheumatism. He had been gradually finding that he could walk with difficulty; suffered from severe pains in the ankle-joints, symptoms which gradually increased until he was unable to walk, and at last there was complete paraplegia—entire loss of motion and sensation in the lower half of the body—and paralysis of the bladder and rectum. While he had been exposed to venereal trouble, there was no history of syphilis. Suspecting, however, that the trouble was of a specific nature, fifteen grains of the iodide of potash with a sixteenth of a grain of corrosive sublimate were given, the iodide being gradually increased until two drachms were taken daily. Within two months the patient began to regain control of the extremities and of the bladder and rectum; after two years he considered himself in excellent health, and got married, and had remained healthy since, until very recently, when he had bronchitis and suffered, as was stated, a good deal from his old rheumatism.

The second case was that of a man of 55 years, who had had syphilis in early life, but had remained in good health until the ataxic symptoms began to develop. He was put upon anti-syphilitic treatment, and recovered from the ataxic symptoms.

The third case was that of a surgeon in the late civil war, who two years ago began to suffer from difficulty in locomotion, and finally paralysis on the left side. He denied having had primary disease; but, being thoroughly convinced that his condition was due to syphilis, Dr. Barker gave iodide of potassium and corrosive sublimate, and, as in the other cases, the incidental treatment which symptoms might demand. At that time there was paralysis of one side, inability to articulate, and disturbance of the mind. The rapidity of recovery was marvellous: within one month he could walk, the appetite returned, he gained flesh, the mind cleared up, and he was now apparently perfectly well. It would be evident that the President referred to cases in which there were symptoms of locomotor ataxia, without attempting to draw a distinction as to types of the disease.

Dr. R. W. AMIDON wished only to put himself on record as one of those who were opposed to calling typical locomotor ataxia a syphilitic disease. Those who held the view that locomotor ataxia was due to syphilis generally founded their opinion upon statistical data; and, as shown by Dr. Birdsall in a paper before the American Neurological Society last summer, and by others, this was a doubtful foundation. Only about 9.5 per cent. of the cases, according to Dr. Birdsall's

statistics, showed a possible connection between tabes and syphilis. Dr. Amidon had never seen a case of locomotor ataxia, in which he felt satisfied there was sclerosis of the posterior columns of the cord, benefited by anti-syphilitic treatment. He thought that if we wished to acquire light on the subject we should have to look to pathological anatomy. If sclerosis of the posterior columns was syphilitic, it must be a tertiary lesion, and from what we knew of tertiary lesions he thought we could safely say they did not constitute a systematic lesion. Syphilis was more apt to attack the membranes than the substance of the brain and cord, and most authors agreed that in posterior sclerosis the contents of the nerve-tubules were first affected. The course of the disease was against its being syphilitic: it was slow and progressive, while the course of most tertiary lesions was rapid. What experience he had had, both in the way of treatment and study of the literature and the pathology of the disease, all went in one direction,—namely, against the theory that locomotor ataxia was a syphilitic lesion.

Dr. R. W. TAYLOR said he was already on record as an opponent of the theory, founded on statistics, of the causation by syphilis of locomotor ataxia, and he was very glad to hear the cautious manner in which Dr. Weber had handled the subject. He reviewed the statistical evidence offered by Fournier and others, and expressed his opinion that it was not well founded: Fournier, for instance, made a specialty of venereal disease, and naturally if a patient had syphilis and ataxic symptoms he would go to Fournier. Again, those statistics were largely retrospective, and not founded upon a critical investigation into the patient's past history. The lesions of syphilis, as Dr. Amidon had said, usually developed in the membranes of the brain and cord, and did not correspond with the pathology of typical tabes.

Dr. W. R. BIRDSALL heard only the last part of the paper, but said, with regard to the statistics which had been quoted from his paper, that there were forty-two cases of locomotor ataxia in which particular inquiry was made with regard to the presence or absence of syphilis. The number in which there was a syphilitic history was only four, or about 9.5 per cent. The average deduced from the statistics of various authors was about 43 per cent. While we should not ignore statistics, he thought that up to the present they were not sufficiently large to be of very definite value. Another fact might be mentioned,—that Erb did not recognize the duality of the venereal sore; and this would account in part for the large number of cases in which he found a syphilitic history. Still, it was somewhat remarkable, after all allowance had been made, that there should have been evidence of syphilis in so many of the cases reported by Erb and others. He himself was

disposed to think that syphilis might produce such an effect upon the nervous system as to predispose it later, like venereal excess, great fatigue, and other conditions, to spinal sclerosis. It also seemed to him that where there was an incipient sclerosis of the posterior columns syphilis coming on later might be made worse by it. An important point suggested by the President was with regard to what should be called locomotor ataxia. He believed that there was a great variety of affections of the cord in which there was locomotor ataxia as far as the symptoms were concerned, but in which there were also other symptoms, and other portions of the cord were affected than the posterior columns alone,—the lateral columns, for instance, in which there was a variety of other symptoms, which were often confounded with true sclerosis of the posterior columns. He believed that an anatomical division was an important one to make.

Dr. E. L. KEYES thought the statistical element could not be entirely ignored when so many men of intelligence and high position agreed to give it some weight. Although he did not believe that pure and simple sclerosis of the posterior columns, if there was a distinct pathological lesion of that kind which characterized pure locomotor ataxia, could or ought to be caused by syphilis, still he thought it perfectly possible that meningeal and progressive troubles in the lumbar columns and disseminated throughout, might be caused by syphilis, and might be attended by signs of locomotor ataxia. He mentioned one case in which there were unmistakable evidences of syphilis, and at the same time the patient suffered from paraplegia, wasting of the muscles of both lower extremities, inability to walk, paralysis of the bladder, etc. Under anti-syphilitic treatment the wasting of the limbs, and other symptoms of syphilis, disappeared, but the symptoms due to the ataxic lesions continued, and he died. Another case, also he treated for syphilis, and cerebral symptoms due to the lesions of that disease disappeared. Since then distinct ataxic symptoms had appeared. He proposed to put the patient upon active treatment. He was inclined to attach some weight to the idea that pre-existing syphilis might be an active factor in the production of ataxic symptoms. He had seen two or three cases greatly improve under the use of iodide of potassium and inunctions of mercurial ointment. One patient took the treatment with the iodide of potassium badly, and a change was made to mercurial inunctions, etc., and he then markedly improved in the ataxic symptoms and in general health.

Dr. ALEXANDER McLANE HAMILTON thought a distinction should be made between posterior spinal sclerosis and cerebro-spinal syphilis. In the cases in which there were the classical symptoms of true locomotor

ataxia he had not found a history of syphilis in more than twelve per cent. In the irregular ataxic cases, however, in which there were head- and speech-symptoms, etc., syphilis played an important part, and these cases were greatly benefited by iodide of potassium, —if necessary given in very large doses, two or three hundred grains a day in a mild alkaline water.

Dr. F. N. OTIS thought we should not ignore the large statistics given by different authors, going to show the presence of syphilis in so many cases of locomotor ataxia. His own experience had been comparatively small, but it had led him to believe that locomotor ataxia could be more readily produced in the way in which some syphilitic troubles were produced than in any other way. Looking upon it as a disease of the connective tissues, a sclerosis from cicatrization of connective-tissue accumulation, producing strangulation of the nerve-fibres, it would be found that the disease which produced locomotor ataxia acted exactly as we knew syphilis to do, and in the same rotation. If it were said that anti-syphilitic treatment often had no effect upon tabes, it could also be said that it did not greatly affect certain syphilitic lesions which had gone to an advanced stage. He related a case in which the patient was sent by him to an eminent specialist in diseases of the nervous system, who pronounced his condition undoubtedly one of locomotor ataxia, and advised ergot, and, later, galvanism. As the ergot deranged digestion, Dr. Otis finally concluded to give anti-syphilitic treatment, and also divided a constricted meatus. The patient was soon able to walk with scarcely any difficulty, and his bladder-trouble almost entirely disappeared in a few weeks. He thought it safe always to try iodide of potassium and mercury, as it was perfectly safe to do so even for diagnostic purposes.

Dr. E. C. SPITZKA said an important point had been overlooked to-night,—namely, that syphilis undoubtedly affected the central nervous axis in its secondary period. He had known it in a few cases, and others had noted the same fact, to abolish the tendon reflex. If syphilis in the secondary stage was capable of producing this effect upon the nervous system, might it not produce the same effect in the tertiary period in the manner in which this symptom appeared in locomotor ataxia? It had been asserted that the lesions of syphilis were characteristic and distinct and had not been found in locomotor ataxia. He would deny that *in toto*. He believed it to be perfectly well established that syphilitic meningitis differed in no respect from ordinary meningitis, and most recent investigators upon the subject also denied that any marked line could be drawn in the lesions of locomotor ataxia. In his private practice, where he could rely upon the statements of his patients, he estimated that two-thirds of

the cases of locomotor ataxia had a syphilitic history. It had been shown in the discussion to-night that where there was a history of syphilis it nearly always occurred some years before the development of locomotor ataxia, and this we would expect, for if specific disease could give rise to tabes it would do so in its tertiary stage. Again, locomotor ataxia occurred more commonly in that class of the population where syphilis was most frequent,—namely, among the inhabitants of cities. He was of the opinion that syphilis was the etiological factor in about one-third of the cases of locomotor ataxia.

NEW YORK PATHOLOGICAL SOCIETY.

A STATED meeting was held March 26, 1884, GEORGE F. SHRADY, M.D., President, in the chair.

WEIGART'S METHOD OF STAINING THE CENTRAL NERVOUS SYSTEM FOR MICROSCOPICAL EXAMINATION.

Dr. T. M. PRUDDEN presented some specimens of the spinal cord, a part of which illustrated the lesions of locomotor ataxia, stained with acid fuchsin or the sulphate of rosanilin, after the method of Weigart, of Leipsic, a method which had not yet come into common use in this country and England, or even in Germany. It was of special advantage in studying the nervous system. All the parts were stained alike red by the agent, but afterwards when treated with a dilute solution of caustic potash the color was destroyed in some of the elements sooner than in others. The axis-cylinders retained the color longer than other parts, and in some of the specimens presented they could be very distinctly seen as red lines standing out on a colorless background. The reagent had to be quite pure, and he had been unable to obtain any in this country which would answer the purpose.

COLLOID CANCER OF THE PERITONEUM.

Dr. H. N. HEINEMAN presented the specimen on account of its great thickness and extent. The portion of peritoneum presented which had undergone colloid degeneration was more than two inches in thickness. The patient was 40 years of age. There was no clinical history.

SACCULATED EMPYEMA.

Dr. HEINEMAN also presented a specimen removed from the body of a man who died the evening before at the Mount Sinai Hospital, having been admitted March 15. He was 45 years of age. The health began to fail a year ago, and he suffered more or less from dyspnoea and cough with mucopurulent expectoration. A short time previous to admission he was seized with shortness

of breath, pain, muco-purulent expectoration, a severe chill followed by sweating, and afterwards almost regularly every afternoon there was rise of temperature and sweating. The cough and dyspnoea increased, the health rapidly failed, and he was obliged to cease work. His physician treated him for pneumonia. After admission the afternoon temperature during the remaining days of life varied between 102.5° and 104° F., the pulse rapid, the respiration from 48 to 50; there was a less elevation of the morning temperature. He continued to grow weaker, and three days before death oedema set in, and death took place from asthenia and oedema of the lungs. At the autopsy the pleura were found thickened and adherent, and on one side enclosed two sacs, non-communicating, filled with pus, and another similar sac at the diaphragmatic portion. There seemed to be no channel by which the pus contained in the three separate cavities could have gotten to the bronchi. The other lung was emphysematous and the seat of chronic bronchitis. The heart was somewhat fatty, the aortic and mitral valves thickened, the kidneys the seat of slight chronic diffuse nephritis. It would be interesting to know just how the pleuritic lesions took place. Dr. Heineman thought it probable the man had had empyema, which tended towards recovery, but that relapses took place and pus was enclosed between the adhesions, forming the cavities referred to.

Dr. E. G. JANEWAY had seen two similar cases during the present year. One followed pneumonia. The house surgeon would draw off about four ounces of pus from one sac, and, failing to get more at that locality, would insert the needle at another point and draw off perhaps four ounces more, and in this way the presence of several pleuritic sacs separate from one another and containing pus was demonstrated. A second case began with hydro-pneumothorax, and the patient finally recovered. In a third case, in which an autopsy was made last week, hydro-pneumothorax had developed quite suddenly.

HORSESHOE KIDNEY THE SEAT OF CHRONIC DIFFUSE NEPHRITIS.

Dr. F. FERGUSON presented the specimen, which was removed from the body of a man who was brought to the hospital on the 18th of January, with the statement that he had been suffering for three or four days with dyspnoea, and had been using opium for the last five years. At that time he was taking fifteen grains daily. The dyspnoea on admission was extreme. The urine contained thirty per cent. of albumen. The aforementioned lesion was found.

CONGENITAL ABSENCE OF THE LEFT WING OF THE DIAPHRAGM.

Dr. FERGUSON presented the specimen,

removed from the body of a colored infant at the Morgue. The stomach, the left lobe of the liver, and the greater part of the small intestine and cæcum, were all in the left pleural cavity.

ABSENCE OF THE CORPUS CALLOSUM.

Dr. FERGUSON presented the specimen, in which the corpus callosum was found divided throughout its entire length, and the hemispheres were united at this locality only by the presence of the pia. The patient was about 45 years old, was very small, and died intoxicated. No further history was given.

HEMORRHAGE INTO THE PONS.

Dr. FERGUSON also presented the following case, and presented specimens. The patient was brought to the hospital by the ambulance on the 16th of March. He had worked out of doors. The friends said he had been paralyzed for twenty-four hours and become comatose. The surface was dry, the muscles relaxed, the pupils equally contracted and irresponsive to light; the bladder was distended with urine, which was withdrawn and was found to contain 10 per cent. of albumen; the temperature was 100° F., the pulse 80, the respiration slow and stertorous. Pilocarpin and hot-air baths failed to produce diaphoresis. The patient partially recovered consciousness. The reflexes were lost in the afternoon. The patient died on the same day. At the autopsy the ordinary lesion of chronic diffuse nephritis was far advanced, and the condition of the heart invariably associated with chronic diffuse nephritis was present. A considerable hemorrhage was found in the pons, raising the pia mater within the ventricle and between the medulla and the lobes of the cerebellum. It was only the second instance in which Dr. Ferguson had seen hemorrhage in this locality.

Dr. HEINEMAN recalled a case in which Dr. Alonzo Clark had diagnosed absence of a part of the diaphragm from the tympanic resonance given by the intestines within the thoracic cavity.

Dr. AMIDON said Dr. Wilder had presented the brain of a cat at the American Neurological Society, in 1883, in which there was absence of the corpus callosum, and referred to the case of a man in which there was a similar abnormality, and it had only been recorded of him that he was very profane and quarrelsome.

Regarding the fourth specimen, Dr. JANEWAY said it was commonly noted in books on diagnosis that the presence of albumen in the urine was one of the evidences of uræmic coma versus coma due to cerebral hemorrhage. As a matter of fact, however, it was quite common to find Bright's disease and albuminuria present in cases of cerebral hemorrhage.

CHRONIC PROLIFERATED PERITONITIS.

Dr. L. H. SAYRE presented the specimen for Dr. L. A. SAYRE, and read the history. The patient was 66 years of age, a large and robust man, always healthy until 1876, when he suffered from a severe attack of pleuro-pneumonia, after which he was never entirely well. Two years later he fell and sustained a severe concussion of the spine, followed by complete paralysis of the lower extremities and bladder, symptoms which shortly disappeared. In 1879 he fell again from a hammock, and afterwards suffered from intense pain across the stomach, extending over the abdomen, pointing to acute peritonitis. For the remaining six years of life he took daily hypodermic injections of morphine, to relieve pain. Swelling of the limbs and ascites developed, and a number of times tapping was performed and large quantities of fluid were withdrawn. Only once or twice was a trace of albumen found in the urine. The quantity of urine passed daily since 1879 gradually diminished from year to year, until during the last forty-eight hours of life it amounted only to seven ounces, and the patient died of uræmia. Dr. Welch made the post-mortem and microscopic examinations, and, in addition to the lesions of peritonitis, found lesions in the heart, pericardium, lungs, pleura, liver, spleen, kidneys, and bladder, and atheromatous degeneration of the arteries. The brain and cord were not examined.

DIPHTHERITIC DYSENTERY FOLLOWING LABOR.

Dr. E. G. JANEWAY presented the large intestine the whole length of which was involved by diphtheritic inflammation, probably of not more than four days' duration. The interest of the case was that it should happen just after confinement. The woman was 23 years of age, a domestic, who was delivered of her first child on March 18, at 6 P.M., labor being normal and lasting six hours. The temperature on the following morning was 99° F., and remained at that point until the afternoon of the second day, when it suddenly went up to 104.4°, and the uterus, which had contracted down well after delivery, increased to a large size, extended above the umbilicus, and was flabby. There was a discharge from the vagina, somewhat colored, which was increased by pressure upon the uterus. There was no perceptible odor. Intra-uterine douches of bichloride solution, one to two thousand, were given, when the temperature rose. On the night of the 22d she had a number—perhaps eight—of diarrhoeal movements, which were afterwards controlled by suppositories of opium on the last day of life. Although the temperature continued high and the heart beat comparatively strong, there was no pulse at the ankle or wrist, and the hands were cold. She had taken full but

not excessive doses of ergot; she had had morphine, whiskey, and quinine. There was at the autopsy found to be some diphtheritic endometritis and some patches on the vagina, but the lesions were not so striking in the genital tract as in the large intestine. There were no other lesions except slight parenchymatous nephritis and a large, soft spleen. The case was interesting as showing that in some of these cases the intra-uterine injections will not touch all the disease. In this case the lesions in the intestine had the appearance of being older than those in the uterus. It was further interesting as illustrating a point which Dr. Janeway had frequently marked,—namely, that simple inspection of the discharges will not always enable us to determine the absence of a serious diarrhoeal disease, and even should a little blood or a few clots be found in the stools after pregnancy they might readily be attributed to the lochia.

TUBERCULAR PLEURISY.

Dr. JANEWAY also presented a specimen removed from a patient who had suffered from tubercular pleurisy with absence of tubercles in the lungs and other portions of the body.

DEATH FROM PERITONITIS WITH OBSCURE SYMPTOMS.

Dr. J. LEWIS SMITH was called to see a woman on the 16th of March with the history that during two months she had suffered from a dull, aching pain low down in the abdominal cavity on the right side. Just before he saw her the pain became more intense, and extended over the abdomen. He found general tenderness over the abdomen, most marked in the right iliac region. The temperature, however, was normal, and the pulse not accelerated. Thus peritonitis seemed to be excluded. The temperature was normal also on the following day; no marked acceleration of the pulse; the respiration normal. On the next day she vomited occasionally; the temperature was elevated, but not more than two degrees, and during her whole sickness the temperature did not rise above 102.5°; the pulse about 100. Her monthly flow took place about a week ahead of time, which was attributed to poulticing. Perforation of the appendix was thought of, but she stated that she had had similar pains before at the time of the monthly period, and the symptoms seemed to exclude peritonitis. Some days passed, the symptoms remaining about the same, until suddenly she went into collapse and died. There had been no meteorism. Opening the abdominal cavity, fetid pus was encountered of exceedingly disagreeable odor, which had escaped from a ruptured abscess, and a perforation was found near the middle portion of the vermiform appendix, the walls surrounding presenting a gangrenous appearance. The intestines were matted together by

very intense recent adhesive inflammation. It seemed very plain that for two or three months there had been an abscess, probably due to inflammation excited by a foreign body in the appendix. The odor was so offensive that it prevented a search for the foreign body. Dr. Smith said that in nearly all the cases of perforation of the appendix which he had seen it had occurred near the middle, and not at the extremity. Had the diagnosis been made early enough, the patient might possibly have been saved by opening the abscess from without. Dr. Smith remarked that he once witnessed a case in which the patient died a few days after labor, and it was found that there had been rupture of a perityphlitic abscess started by a baked bean within the appendix.

Dr. FERGUSON had seen a case in which the extremity of the appendix was necrotic.

Dr. W. M. CARPENTER said it had been stated by one physician that the extremity of the appendix in those cases in which a foreign body entered it had previously become bound down in an abnormal position, giving more ready access to foreign material from within the intestine. He had not been able to confirm the statement.

Dr. JANEWAY said it was a mistake to suppose that the vermiform appendix, the seat of a foreign body, always perforated very easily. He had seen one case in which it had become distended to the size of a child's head, and in another to the size of a Bologna sausage.

PHILADELPHIA CLINICAL SOCIETY.

STATED MEETING, FEBRUARY 22, 1884.

The PRESIDENT, HENRY BEATES, JR., M.D., in the chair.

AFTER the reading of a paper on "Infantile Palsy" (see page 501), by G. Betton Massey, "A Case of Puerperal Eclampsia in a Primipara of Eighteen Years of Age" was reported by Dr. JAMES B. WALKER:

Mrs. H. P., primipara, 18 years of age, of full habit, short stature, florid, and always healthy; had been under observation for about six weeks prior to labor. The urine had been repeatedly examined, with negative results. Had had but little nausea during the early months, and absolutely no bad feelings otherwise until about a week prior to labor, during which there was considerable general itching of the surface of the body. There was no jaundice. The feet were very slightly œdematous during the last two or three days. She was directed during the last two or three weeks to eat sparingly, for her appetite was keen; but I doubt if the advice was very influential in shaping her practice.

On the night preceding December 20, 1883, she slept but little, having pains in the back of slight severity. About 5 A.M. I was called, and found that the bowels had been freely

and voluntarily moved, and that she had been freely micturating at short intervals all night.

The os was fully dilated by 10 A.M., and the pains of the second stage commenced, the child presenting in R. O. P. (fourth position), and the bag of waters unbroken. The head was still freely movable at the superior strait, and not forced down into the pelvis, as is natural in primiparæ even at the commencement of labor, when pelvis and child bear normal relations to each other, both as to position and quantity. During the earlier "pains" of the second stage the child's head was altered, and the anterior fontanel presented in centre of strait, the brow under symphysis pubis; the occiput had ascended posteriorly, carrying the posterior fontanel out of reach. This was corrected by carrying the brow above symphysis and forcing it around to the left, with two fingers to the sides of the head, when the occiput again presented in the fourth position. The head being large and the young patient being scarcely fully developed as yet, descent was very slowly made, delays marking each stage of progress, the pains, except when the head was at the vulva, being short-lived. A mixture of ether, alcohol, and chloroform, in three, two, and one parts respectively, was allowed during some of the severer pains of the second stage.

The head was delivered at 2.30 P.M., by enucleation, during an interval of "pains," without injury to the perineum. During the delivery of the head and the last two or three pains prior to its delivery, the patient became considerably excited, although easily controlled.

The cord being tied, attention was addressed to the uterus and its contents. The secundines were removed easily by the Crêdè method, and just as they escaped from the vulva my attention was directed to the patient's head, which was slightly turned to the right, with the eyes markedly in the same direction and fixed. I spoke to her, but she did not notice it, and I at once administered the chloroform mixture. The convulsive movements were altogether confined to the neck and face, the extremities not participating, and the facial movements being very slight. Loss of consciousness was, however, complete. No stupor succeeded. A hypoderm of one-fourth grain of morphia was at once administered, and a dose of potass. brom., gr. xx, and chloral, gr. x, given.

In a half-hour, while patient was sleeping, a second convulsion, about equal in mildness to the first, occurred. This was at 3 P.M. The chloral and bromide mixture was now repeated, and continued every two hours, and alternated hourly with one of bromide of potass., acetate of potass., and mist. potass. citratis. The urine, on being drawn with a catheter at 4 P.M., was very slightly albuminous. The patient continued quiet, doing

nicely,—sleeping under the medicines, but rational on waking,—until 7.30 P.M., when a short visit from her somewhat boisterous father, made without advice, excited her, and a third convulsion, general in character and of decided severity, occurred. This, though short-lived, was followed by coma. A second hypodermic of one-fourth grain of morphia was now given.

I now requested counsel, and Dr. Tyson was sent for. He arrived at 8 P.M., just as the patient went from her comatose condition into the fourth convulsion, which was about equal in severity to the third. Her face was florid; pulse 120, and not very full; temperature 100.2° F.; and, as she had lost but a small amount of blood with the labor for one so full of it, bleeding was decided upon, and she was bled from the right arm two full pints. The blood flowed very freely and clotted firmly, leaving but little serum. Stupor lessened after this, but consciousness did not return, though she swallowed what was given her to take, consisting of the medicines above mentioned and water freely given after each dose, as had been practised from the time of the first convulsion. Elaterium was administered in quarter-grain doses every three hours till purging induced. Free purgation followed the third dose. Digitalis was given after the bleeding, and repeated in ten-drop doses every hour for the first fifteen hours; then, as the pulse had dropped to ninety-six beats, five drops every hour for twelve hours; and after that to convalescence ten drops four times a day. A large poultice of flax-seed-meal was applied to the loins.

A fifth convulsion occurred at 11 P.M., three hours after the last, and about equal to it in severity.

Urine was now drawn by catheter and found *densely* albuminous. The following morning it was still *densely* albuminous, and urates were copiously precipitated on its cooling. No further convulsions occurred. The urine, drawn by catheter every four hours, showed no change in albumen until Saturday night, when it was notably lessened.

On Friday P.M., microscopic examination of urine drawn in the morning showed blood-corpuses and a few coarsely granular and hyaline casts.

None of the chloral mixture was given after 4 P.M. on Friday, but the bromide and acetate mixture, with digitalis, was continued in diminished doses until Saturday noon, after which a potash mixture, without bromide, was given for several days, with digitalis, as already mentioned.

By Saturday noon, in response to questions as to her feelings she would attempt to answer, but, failing to get the word she wished, would seem puzzled. Mental effort was prohibited. Absolute quiet enforced from the commencement. The babe, which had been sent to a distant room after the first convul-

sion, was fed artificially until Saturday P.M., when, as the breasts were enlarging, it was allowed to nurse and at once removed. On Sunday it nursed every four hours, but was not allowed in the room in the intervals until Wednesday. On Monday and thereafter it was nursed every two hours during the day-time.

Complete consciousness returned Sunday noon, after a quiet, refreshing sleep of several hours.

The urine secreted on the days succeeding labor amounted to about twenty ounces in the twenty-four hours, still containing albumen and urates, but in diminishing amounts, until Friday, the 28th, when not a trace of albumen was present. It had now increased to about thirty to thirty-five ounces in the twenty-four hours. It was repeatedly examined, during the next three weeks, both for albumen and for tube-casts, with uniformly negative results.

Patient first expressed desire for food on Monday, the 24th. Milk and beef-tea were administered on Sunday, 23d, and thereafter.

Thursday, 27th, she expressed a desire to sit up, and was surprised at not being permitted to, as she felt so thoroughly well. She has had no trouble since, having made a rapid and complete convalescence, and exhibits a worthy pride in her bouncing girl baby.

In reviewing this case, several points present themselves for consideration. Had I had reason to anticipate convulsions, I would have given anæsthetics more freely during the labor, and would have delivered with the forceps earlier, thus avoiding the long, tedious strain on a nervous system still possessing somewhat of the convulsibility of childhood. But, as I had repeatedly examined the urine, even within a few days, without result as to albumen, I did not anticipate renal trouble; and, further, urine was voided frequently and in proper quantity up to the commencement of the second stage, four and a half hours before the termination of the labor. Query: Was the cutaneous itching suggestive of retention in the blood, and attempted elimination by the skin, of materials of tissue-metamorphosis which should have been removed by the kidneys? and, if this symptom exist in a case, especially in a primipara, should it be considered suggestive of a danger of convulsions? and, thus forewarned, may the physician be forearmed, even in the absence of albuminuria?

Again: Had I administered a nitrite and induced uterine hemorrhage (which I tried to favor by omitting ergot and ceasing palpation of the uterine globe), might I have prevented further convulsions?

The necessity and advantage of absolute quiet were both illustrated by this case. I feel sure, from the manner the patient was improving, that the third convulsion would not

have occurred had her father not made his boisterous visit. And I am equally certain that the absence of the baby from the room, and other methods of enforcing quiet, aided materially the quiescence of nerve-centres, and thus avoided further dangers.

Still again: What is the pathology, so far as the renal tissue is concerned, of such an attack? I do not believe it to have been acute inflammation of the renal tissue, or, in other words, acute Bright's disease. I rather look upon these cases as due to acute renal congestion. I have no doubt that many cases, perhaps the majority, of primiparæ, have the same condition, but, in the absence of the essential amount of emotional excitement, the convulsions do not occur, the retention of excrementitious substances adding to the convulsibility, but not being in itself sufficient to induce convulsions. That such cases as I have described may develop Bright's disease in its chronic form, I have no doubt; and I think we cannot be too guarded in protecting against muscular efforts and exposures incident to early getting about after lyings-in accompanied by convulsions of this character, even though Bright's disease have not pre-existed. I would even restrict the amount of animal food ingested, especially of meats, for some days after other contra-indications for them have passed, on account of their known (or shall I say apparent?) influence in favoring the dropsies and hence renal difficulties following scarlet fever.

The case illustrates the advantage of blood-letting, I think, in a marked degree. Other measures for placing centres at rest and establishing the renal function were failing, and the patient was fast passing into a coma which required but a few more repetitions of the convulsions to deepen into death.

The slow reaction of the kidneys, which for at least five days thereafter, despite the potassium salts and digitalis and the copious injection of water, amounted only to twenty ounces in the twenty-four hours, proved to me how futile these measures would have been, unaided by the blood-letting, in controlling the convulsions.

(Discussion will appear in our next issue.)

AINHUM.—Dr. L. A. Duhring (*American Journal of the Medical Sciences* for January, 1884) reports a case of ainhum, of which but few have been reported in our country. Its geographical distribution includes chiefly the west coast of Africa, and certain localities in South America, more particularly Bahia, Rio de Janeiro, and Buenos Ayres. As the disease becomes better known, it will, doubtless, be found that it is met with throughout our Southern States, though probably as one of the rarer diseases. Appended to the paper is an exhaustive study of the microscopic appearances.

REVIEWS AND BOOK NOTICES.

A SYSTEM OF ORAL SURGERY: BEING A TREATISE ON THE DISEASES AND SURGERY OF THE MOUTH, JAWS, FACE, TEETH, AND ASSOCIATE PARTS. By JAMES E. GARRETSON, M.D., D.D.S., etc. Illustrated with Numerous Steel Plates and Wood-Cuts. Fourth Edition, thoroughly revised, with Additions. Cloth, 8vo, pp. 1037. Philadelphia, J. B. Lippincott & Co., 1884.

This valuable work of Prof. Garretson appears in its fourth edition in an enlarged and more complete form, every part showing careful revision, while in many points such additions have been made as to constitute it a real encyclopædia of oral surgery. Its sixty-seven chapters contain a *résumé* of the subject of dentistry and allied subjects, making it useful both as a text-book for students and a work of reference for medical practitioners. Commencing its consideration of the subject with the anatomy of the parts concerned, it proceeds to dental physiology, the lesions of first and second dentition, anomalies, pathology of dental caries, and dental therapeutics. The various procedures of operative prosthetic dentistry are taken up systematically in several chapters, in which the instruments and methods of use are very fully figured and explained. Diseases of the face, nose, mouth, and pharynx are then rapidly reviewed, including skin affections, neuralgia, and morbid growths. The work concludes with a comprehensive chapter upon anaesthesia and two upon inflammation and diagnosis, the latter being written especially for students, who will find in them much that is suggestive and useful.

The work is abundantly illustrated, the cuts varying in style, in execution, and in value: some seem to be unnecessarily introduced. Figs. 70 and 71 are the same as 104 and 105, and Figs. 51 and 52 are identical with 264 and 265; but this may have an especial convenience to students. The statement that Fig. 105 (page 269) represents the "typical American mouth" we feel inclined to take exception to, as it represents only the superior dental arch, and contains only two incisors, the lateral incisors being absent: this condition certainly cannot be so common as to give it a typical character. We regret that its author, in the chapter on anaesthetics, has not taken positive grounds against the use of chloroform by dentists; and we cannot endorse the statement that this agent may be given with safety in the sitting position if there is no evidence of heart-disease present in the patient. This is dangerous doctrine, for, apart from the difficulty in recognizing the existence of a fatty heart (which only an expert physician could pronounce upon), there is abundant evidence that chloroform can kill suddenly by its direct

action upon the cardiac tissues, even when the heart is to all appearance healthy. Dentists should take it as an axiom that the habit of administering chloroform with the patient in a sitting posture must sooner or later lead to a coroner's investigation; and coroners at the present day do not look upon the administration of chloroform with a great degree of favor. In this opinion they have the support generally of the medical profession. No chloroform in dentistry, and as little of it as possible in general surgery, is the only safe rule; for, no matter how much care be exercised, accidents will occasionally happen.

LEGAL MEDICINE. By CHARLES MEYMOTT TIDY. Vol. III. New York, William Wood & Co., 1884.

This book is the continuation of the medico-legal treatise, by Dr. Tidy, already noticed as to its first two volumes in this journal. The present volume treats of all matters related to the function of reproduction, and also of asphyxia and the various forms of death in which it occurs. The portions treating upon sexual crimes and allied questions seem to us very full, but we notice that the author has apparently never seen the article upon the age-limits of fertility in the female, published in the *Philadelphia Medical Times* some years since, by Dr. Fordyce Barker,—an article which is almost unrivalled in the literature of its class. A very important feature of this volume, as of the other two, is the long analytical lists of illustrative cases, which make the book valuable not only as an authority which may be studied and quoted from, but also as a work of reference out of which may be cited that which is so dear to the heart of the legal mind,—namely, cases.

H. C. W.

GLEANINGS FROM EXCHANGES.

CLINICAL HISTORY OF A CASE OF RECURRENT DROPSY OF THE LEFT MIDDLE EAR, COMPLICATED, AFTER EIGHT YEARS' DURATION, BY AN ACUTE ATTACK OF MONOCULAR OPTIC NEURITIS (CHOKED DISK) ON THE SAME SIDE, FOLLOWED BY GENERAL TABETIC SYMPTOMS.—Drs. Burnett and Oliver record (*American Journal of the Medical Sciences* for January, 1884) a very rare case, which is as yet unfinished. They enumerate the four following factors of disease as being the cause of the patient's condition:

First. The early appearance in life of a polypoid growth in the right ear, which, upon being extracted, caused obliteration of the right auditory canal, and destruction of the contents of the middle ear. A partial loss of motility of the facial muscles upon the same side took place at the same time through peripheral traumatism, although this was not

of sufficient moment to cause interference with their electrical reactions.

Second. A chronic catarrhal inflammation of the left middle ear, of an exceedingly rare and almost unique type,—the peculiarity consisting in the formation and the retention of a sero-mucous fluid within the tympanic cavity, which required frequent evacuation,—each operation being productive of a marked increase in audition; the tendency to the accumulation of the liquid at last ceasing, with a return of the power of hearing to a relatively normal standard; this being purely local in origin.

Third. This conclusion is divided into two probable conditions, there not being any proof of an individual or a combined existence.

A. A chronic pachymeningitis, limited to the anterior two-thirds of the left base, involving a few of the nerve-sheaths at their foramina; causing subvaginal œdema, with consecutive neuritis and partial atrophy.

B. A new growth, very chronic in its development and course, situated in any part of the brain. The neoplasm causing pressure in all directions, with accidental passage of arachnoidal fluid through a few of the weaker foramina into the outgoing nerve-sheaths; this serous exudation producing incomplete choking of the nerve, followed by inflammation and atrophic degeneration.

Fourth. Sclerosis of the posterior columns of the spinal cord; the disease having advanced as far as the beginning of the stage of full development, without complication or extension of morbid process.

CASE OF FRACTURE THROUGH THE ANATOMICAL NECK OF THE HUMERUS, WITH COMPLETE SEPARATION AND DISLOCATION OF THE HEAD OF THE BONE INTO THE AXILLA, WITH ITS SUBSEQUENT REMOVAL.—Dr. Thomas G. Morton reports an interesting case in *The American Journal of the Medical Sciences* for January, 1884, in which the line of fracture was directly across the anatomical neck, where some capsular tissue remained attached. The humerus showed considerable absorption and rounding off at the seat of fracture, but the joint was otherwise uninjured. The removal of the head of the bone was deemed advisable, as it was acting as a foreign body. A good result was obtained.

CONVENIENT WATER-TEST.—Dr. E. Gregory commends the following test. Make a solution according to the following formula:

Liq. potassæ, parts 70;

Aque destillatæ, parts 90;

Potassii permanganat., part 1, by weight.

This solution is to be kept in a colored glass hydrocyanic acid vial with a glass stopper. In applying the test, two test-tubes are to be taken of about four inches in length by half an inch in diameter. These are each two-thirds filled, one with the water to be tested,

and the other with some standard water of known good quality. Into each is dropped one minim of the test-solution and thoroughly mixed. The tubes are then to be stopped with cotton-wool and set aside in the rack, with a sheet of white paper behind them. The freedom of the water from organic impurity seems to be in direct proportion to the time it retains the pink color of the permanganate. A water of average good quality should keep its color well for forty-eight hours. If decidedly paler in twenty-four hours, it is not fit for use. A very slight precipitate occurs in good potable water.—*Druggists Circular*.

ARTIFICIAL COUMARIN.—Coumarin is the odoriferous principle of the tonka bean, woodruff, sweet-smelling grass, and other plants, which owe their aroma and value exclusively to the coumarin they contain, the other principles being not only valueless but disadvantageous. Coumarin has now been made artificially. One method of preparation is by warming sodium salicylaldehyde ($\text{NaC}_7\text{H}_5\text{O}_2$) with an anhydrous acid, or by boiling salicylaldehyde with sodium acetate and anhydrous acid. Recent improvements in the process have greatly shortened the time and expense.—*Pharmaceutical Journal*, September 8, 1883; from *The Practitioner*.

LONG INCUBATION OF VACCINATION.—In the *Druggists' Journal* (Philadelphia) for January, 1884, is reported a case in which a vaccination with bovine virus did not produce any effects until nine months and two weeks, when a typical vesicle appeared, which then went through the regular stages, both local and general, of an ordinary vaccination. The patient was a man who had never been vaccinated before, but consented to be vaccinated when his children were; but although the others, upon whom the same virus was used, passed through the usual course, his case was apparently unsuccessful until the lapse of time already stated, when he came to the physician with a characteristic sore arm.

IODOFORM SUPPOSITORIES FOR PILES.—The following recipe for suppositories for hemorrhoids is from the *Zeitschrift für Therapie*:

R Iodoform	4 parts.
Balsam Peru	8 "
Cacao butter	} of each 6 "
White wax	
Calcined magnesia . .	4 "

Mix. To make twelve suppositories. One to be introduced after stool each time.—*Druggists Circular*.

EXCESSIVE DOSES OF MORPHIA: EIGHTY-FIVE GRAINS IN ONE DAY.—Dr. L. S. Hinkley, of Avon Springs, New York, reports (*New York Medical Journal*, p. 354) a case of a morphia-consumer, a woman, 26 years of age, who took twenty-five grains of sulphate of

morphia hypodermically at one dose, in his presence, without narcotism. She stated that this was her daily dose, and at times she took two such doses in a day. She declared that she had taken as much as a drachm at night and twenty-five grains in the morning, and had continued this quantity daily for nine days, without any very marked effect.

MISCELLANY.

AN IMPOSTOR.—The superintendent of the Eighteenth and Nineteenth Wards District Association of the Philadelphia Society for Organizing Charity requests us to warn the profession against a professional beggar who represents himself as a medical man in distress, and makes a speciality of victimizing physicians. He is of dark, sallow complexion, about five feet four inches high, has very affable manners, and makes abundant promises to return temporary loans of money. He lives in good style upon the proceeds of swindling, and is an impostor, not deserving of sympathy. His name and address have been sent to this office. His name is not in the *Medical Register*, although he claims to be a regular graduate.

THE FIFTY-EIGHTH ANNUAL COMMENCEMENT of the Jefferson Medical College was held on March 29, at the Academy of Music, when two hundred and fifteen students were graduated. The valedictory address was delivered by Prof. John H. Brinton. The trustees conferred the degree of LL.D. upon Prof. J. M. Da Costa, and also upon Prof. Benjamin Silliman, of Yale College. The degree of D.D. was conferred upon the Rev. J. F. Dripps, of this city.

"ROUGH ON RATS" (a compound containing arsenic) has been administered for criminal purposes in New Jersey, where a whole family was made sick, and in North Carolina, where an infant was killed by it.

THE MEDICAL JURISPRUDENCE SOCIETY will hold its next monthly meeting on Tuesday evening, April 8, at the College of Physicians. Henry Hazelhurst, Esq., will read a paper on "Trial by Jury."

THE PROCEEDINGS OF THE MEDICAL SOCIETY OF THE COUNTY OF KINGS has been discontinued as a separate publication, and will appear hereafter in the *New York Medical Journal*.

DR. J. C. DA COSTA has been appointed Gynecologist of the Jefferson Medical College Hospital, to succeed Dr. F. H. Getchell, who resigned a short time ago.

PROF. MICHAEL FOSTER, who has been announced as an expected visitor during the coming summer, informs us that he is not coming.

PROF. BAUDUY, of the Missouri Medical College, has recommended bromidia as a reliable hypnotic and as an efficient substitute for chlorodyne.

At the quarterly stated meeting of the Philadelphia County Medical Society, held April 2, a proposed amendment to the Constitution and By-Laws, making women ineligible for membership, did not succeed in obtaining a two-thirds vote. A woman-candidate for membership was rejected at the same meeting.

NOTES AND QUERIES.

LINNEUS'S "GOLDEN STATUE" FOR DR. PATRICK BROWNE, WHO FIRST INTRODUCED SPIGELIA TO THE MEDICAL PUBLIC.

In looking up some references in the Peabody Library, I was considerably interested in finding in "The Civil and Natural History of Jamaica," London, 1756, by Dr. Patrick Browne, what is probably the first account of the anthelmintic properties of spigelia. This seems probable from a manuscript letter appended to the volume, and written to Dr. Browne by the celebrated naturalist Linneus. It is dated Upsal, 29 October, 1756, and contains the following: "What you have delivered concerning the spigelia against worms, is very wonderful, since the like never was met with in the medical art, for which alone you ought to be honored with a golden statue."

As it may be of interest to the many practitioners who today use this excellent remedy to know something of its history, I append a portion of Dr. Browne's account. "The 'worm grass' grows naturally in most parts of South America, and is now cultivated in many of the gardens of Jamaica. It has been long in use among the negroes and Indians, who were the first acquainted with its virtues, and takes its present denomination from its peculiar efficacy in destroying worms, which I can affirm, from a great number of successful experiments.

"It does this in so extraordinary a manner that no other simple can be of equal efficacy in any other disease as this is in those that proceed from these insects, especially when attended with fever and convulsions. The method of preparing this medicine is as follows,—viz.: You take of the plant, roots and all, either freshly gathered or dry, two moderate handfuls, and boil them over a gentle fire in two quarts of water, until one-half of the liquid is consumed, then strain off the remainder and add a little sugar and lemon-juice to give it a more agreeable taste and keep it from growing viscid or clammy. It may be, however, observed that the decoction is sometimes clarified and sweetened, and is then equally efficacious; which gives a hint to have it made into a syrup.

"The common method of administering this medicine is as follows,—viz.: to a full-grown person you give half a pint at the hour of rest, and a proportionate quantity to all weaker or younger subjects, which is to be repeated once in twenty-four hours for two or three days after. But as the largeness of this dose may render its action too violent, and the use of it both unsafe and precarious, I would recommend the following method as less hazardous and as effectual: Give about four ounces to a grown person for the first dose, and about two or three every six hours after, if its anodyne quality will permit; but to persons of weaker constitution it should be repeated only every ten or twelve hours. This is to be continued for the space of thirty-six or forty-eight hours, when the double dose may be again repeated, and after it takes its full effect it must be worked off with some gentle purgatives, such as the infusion of senna or rhubarb with manna, etc. This medicine procures sleep almost as certainly, and in equal degree with opium; but the eyes seem distended and appear bright and sparkling as they generally do before the eruption of the smallpox and measles, after the sleepy effects are over.

"In a short time after this first dose is administered the pulse grows regular and begins to rise, the fever cools, the convulsions, if any, abate. All the symptoms appear more favorable, and the worms are generally discharged in great quantities by the use of the subsequent purgatives, if not before,—often above a hundred at a time. But when a few only come away, and those alive, which is seldom, the dose must be again repeated, and this scarcely ever fails. I never knew this medicine ineffectual when there was the least probability of success; nay, I have often found it serviceable

when there was not the least reason to expect it. I have been, however, cautious in ordering it for children, for, although I never knew it at all hurtful, its effects upon the eyes are such as frequently to deter me, especially as their fibres are weakly and more sensible of irritation, and the fevers arising from this source in such subjects, seldom so violent as to hinder the administration of some other medicine equally as effectual when the symptoms are not too urgent."

Dr. Browne certainly shows himself to have been a very close observer, and his hints as to the administration of a syrup of spigelia, followed by senna or rhubarb and manna, are carried out to this day,—over a hundred years since his announcement,—with very little improvement.

CHARLES S. DOLLEY, M.D.

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JOHN HOPKINS UNIVERSITY,
BALTIMORE, MD.

A CORRECTION.

DR. CASPER MORRIS, written by mistake "Wistar" in our last issue, was not, as therein stated, formerly a treasurer of the American Medical Association, although he had attended several of its meetings as a delegate. We are happy to state that Dr. Caspar Wistar, the ex-treasurer of the Association, is still with us, and apparently in the enjoyment of good health and excellent spirits, for the continuance of both of which he possesses our very best wishes.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY FROM MARCH 16, 1884, TO MARCH 29, 1884.

HAMMOND, JOHN F., COLONEL AND SURGEON.—Now in New York City, on sick-leave of absence, will, after the expiration of his sick-leave, await orders in that city. Paragraph 8, S. O. 70, A. G. O., March 26, 1884.

BACHE, DALLAS, MAJOR AND SURGEON.—Leave of absence still further extended seven days. Paragraph 1, S. O. 50, Headquarters Department of the East, March 14, 1884.

MATTHEWS, WASHINGTON, CAPTAIN AND ASSISTANT-SURGEON.—To be relieved from duty in the Department of the Missouri, and to report in person to the Surgeon-General of the Army, for duty in his office. Paragraph 12, S. O. 62, A. G. O., March 15, 1884.

GIBSON, R. J., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Relieved from duty at Fort Hays, Kansas, and ordered to Fort Wingate, New Mexico, for duty. Paragraph 3, S. O. 58, Headquarters Department of Missouri, March 18, 1884.

GANDY, CHARLES M., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Assigned to duty at Fort Brady, Michigan, as post surgeon. Paragraph 6, S. O. 56, Headquarters Department of the East, March 22, 1884.

EDIE, GUY L., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Assigned to duty at Fort McIntosh, Texas.

ROBERTSON, REUBEN L., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Assigned to duty at Fort Ringgold, Texas.

Paragraphs 3 and 4, S. O. 33, Headquarters Department of Texas, March 17, 1884.

CROSBY, WILLIAM D., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Relieved from duty at Fort Huachuca, Arizona Territory, and ordered to Fort McDowell, Arizona Territory, for duty. Paragraph 1, S. O. 20, Headquarters Department of Arizona, March 15, 1884.

BLACK, CHARLES S., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Assigned to duty at Fort Concho, Texas. Paragraph 6, S. O. 30, Headquarters Department of Texas, March 10, 1884.

LIST OF CHANGES OF STATIONS OF NAVAL MEDICAL OFFICERS DURING THE WEEK ENDING MARCH 29, 1884.

P. A. Surgeon E. H. GREEN, to the Greely Relief steamer "Thetis."

P. A. Surgeon E. H. MARSTELLER, to the Receiving-ship "Colorado," New York.

P. A. Surgeon I. H. BRYAN, resigned, to take effect April 10, 1885, with leave of absence till that time and permission to leave the United States.

P. A. Surgeon C. W. RUSH, to the Naval Academy.